Political Trust, Corruption and Economic Development in the Former Soviet Space

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Abstract

Does corruption make us less likely to trust political institutions? Comparative investigations of this question have shifted from a resounding "yes" to more uncertain prognoses in the last decade. I engage with theories in political economy to help explain high-trust, high-corruption patterns in the former Soviet states, with minor comparison to Latin America and East Asia. Specifically, I use quantitative methods to investigate whether citizens trade off petty corruption concerns in good economic times, and whether they are equally punitive of corruption across the regime spectrum. Finally, I investigate whether economic growth is a boon or burden for incumbent support across Russian subnational regions. To bolster my empirical approach, I critique existing conceptions of political trust and test for the measurement equivalence of political trust across heterogeneous populations. In doing so, I offer new theoretical insight into how citizens relate to their political institutions in the presence of public institutional dysfunction, and I offer new methodological insight into how we can better measure political trust in cross-regime settings.

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Preface

John Locke argued that government is fundamentally "established by society as a trust" forged from individual consent. In a healthy republic, "arbitrary or absolute power can never be legitimate, consented to, because 'God and Nature' do not allow 'a man so to abandon himself as to neglect his own preservation'" (Pitkin 1965, 994-5). This dissertation concerns the underbelly of Locke's ideal by examining societies where arbitrary power meets consent. As survey data has expanded around the globe in the last decade, we increasingly find that people express support for political institutions even when they face extortion and other forms of discrimination to obtain basic public services. The co-existence of corruption and strong political approval raises interesting questions about how governments across the regime spectrum retain public support in dysfunctional institutional conditions.

I aim to shed light on this phenomenon in rarely examined parts of the Soviet space where rampant corruption thrives alongside comfortable levels of trust in political institutions and rapid economic growth. In this context, I examine the applicability of corruption tradeoff theory, the idea that people trade off concerns about corruption for material gains and thereby help prevent institutional reforms toward the rule of law from the bottom up. I draw on empirical insights about corruption tradeoff in Latin American clientelist democracies, hypothesizing that economic performance moderates the extent to which citizens with high exposure to petty corruption and extortion punish political institutions in their trust assessments. I ultimately find that there is more to the data than meets the eye. Economic performance is not an opiate of the masses and does not weaken the notion that political authorities bear responsibility for petty corruption, not even among citizens living in authoritarian countries where petty corruption has become part of everyday transactional life over multiple generations.

Survey data currently offer our best glimpse into the story of political trust and corruption responsiveness in closed societies across Central Asia and the Southern Caucasus. Although often overlooked as case studies even in the post-communist area studies literature, public opinion patterns in these countries can offer important insights into the study of development, institutional change and political transition. To my knowledge, I make the first attempt in the literature to compare political attitudes in these authoritarian and kleptocratic regimes to their post-communist counterparts in Eurasia and Eastern Europe. By adopting a large N comparative design which includes democracies and non-democracies, I risk using unreliable public opinion data which are neither internally valid in repressive contexts nor externally comparable with other countries. In undertaking this risk, however, I believe that there are certain payoffs in learning not only about the substantive dynamics of trust and corruption in the region, but about the methodological challenges of using survey questions which vary in sensitivity and meaning across space. This dissertation ultimately combines insights about methodology, theory and empirics to enhance our understanding of the political economy of political trust formation and corruption responsiveness in cross-regime contexts.

The dissertation is split into five chapters, moving gradually from an emphasis on methodology to an emphasis on theory and empirics. In Chapter 1, I survey conceptions and measurements of political trust in cross-national survey research, discussing the importance of balancing the demands of local measurement validity and large N measurement equivalence to facilitate the best possible comparative inferences about political trust using flawed and limited data. In Chapter 2, I offer an empirical test of the cross-national measurement equivalence of political trust in my sample. I show that averaging heterogeneous sets of survey questions can mask important differences in how people interpret those questions across the regime spectrum. In

Chapter 3, I introduce the theory of corruption tradeoff and discuss how it can potentially explain high trust and corruption concurrence in the former Soviet space. I outline potential causal mechanisms and measurement choices in my empirical tests of the theory, which follow in Chapter 4. I finish the dissertation with a case study in Chapter 5, where I analyze the influence of economic performance across time and space on incumbent approval in Russia, commenting on the different influences of subjective and objective economic indicators on political attitudes and what they might teach us about political legitimacy in hybrid authoritarian regimes. In the conclusion, I discuss the implications and limitations of the dissertation and offer suggestions for future research.

1 Chapter 1. What is Political Trust and How do we Measure it Cross-Nationally?

1.1 Introduction

If we ask someone to rate her trust in government on a scale from 1 to 5, what are we measuring, and how do we interpret responses to this question across countries? My goal in this chapter is to examine the conceptual and methodological challenges of studying political trust cross-nationally. To better understand what we can meaningfully infer from survey measures, I want to engage three related questions: 1) what does political trust *mean*? 2) how *valid* are survey measures of political trust? That is, how closely do measures correspond to what they mean? And 3) how *equivalent* or *comparable* are survey measures of political trust across countries? Do survey respondents around the world have similar things in mind when evaluating political institutions?

Although the empirical literature is fairly unclear about the meaning of political trust, it appears to me that measures of trust in government are capturing three possible things: 1) a broad concept of trust as an expectation of political performance, 2) a specific, normative conception of trust as an expectation that political authorities will act in accordance with citizens' wishes, or 3) a proxy for political trustworthiness or political legitimacy. In the first part of this paper, I draw on an interdisciplinary social science literature to consider the meaning of each concept in an effort to better pin down what we are measuring empirically. I wish to argue that we are measuring neither a broad concept of trust nor political legitimacy, despite the heavy use of such terms in the empirical literature.

I believe we are describing a specific, normative conception of trust which is empirically difficult to capture. In the second part of the paper, I wish to examine more closely how we might better operationalize political trust. There is no easy way to do this because extant survey questions

rarely inquire into what standards people use to evaluate political institutions. Since I am currently unable to commission more sophisticated cross-national surveys to alleviate these concerns, I will discuss the importance of balancing the demands of local measurement validity and large N measurement equivalence to help us make the best possible comparative inferences about political trust with the flawed data that we have.

1.2 What is Trust?

In its broadest sense, trust is synonymous with *expectation.* For Dasgupta (1988, 51), trust can be defined "in the sense of correct expectations about the *actions* of other people that have a bearing on one's own choice of action when that action must be chosen before one can *monitor* the actions of those others. Luhmann (1979, 39) writes that trust is "an attitude which allows for risk-taking decisions" and more explicitly "the generalized expectation that the other will handle his freedom, his disturbing potential for diverse action, in keeping with his personality – or, rather, in keeping with the personality which he has presented and made socially visible." Barber's (1983, 9) widely cited definition refers to trust as "the expectations, which all humans in society internalize, that the natural order – both physical and biological – and the moral social order will persist and be more or less realized." His three modes of trust include the expectation of the "fulfilment of the natural and moral social orders," the expectation of "technically competent role performance" and the expectation that "partners in interaction will carry out their fiduciary obligations and responsibilities" (ibid.). Fukuyama (1995, 26) follows Barber to some degree in defining trust as "the expectation that arises within a community of regular, honest, cooperative behavior, based on communally shared norms, on the part of other members of that community."

In these general accounts, trust is an expectation that someone else's behavior will be consistent over time and appropriate to its context.

Others like Hart (1988, 187) and Dunn (1988, 80) define trust as a means of coping with the freedom of others. Gambetta (1988, 218) defines trust as a "particular level of the subjective probability with which an agent assesses that another agent or group of agents will perform a particular action, both before he can monitor such an action (or independently of his capacity ever to be able to monitor it) and in a context in which it affects his own action." Trust operates as a "threshold point, located on a probabilistic distribution of more general expectations, which can take a number of values suspended between complete distrust and complete trust, centered around the midpoint of uncertainty" (ibid.). Sztompka (1999, 25) follows a similar conceptualization as an evaluation of probability, defining trust as a "bet about the future contingent actions of others." Offe's (1999, 47) interpretation also echoes this idea: "trust...is a reflectively fallible ex ante guess based upon an assessment of others." Trust is different from attitudes like *faith*, which tends to indicate complete uncertainty or "blind trust," and *confidence*, which indicates something closer to complete certainty, little vulnerability and little confrontation with specific trustees or tasks (Luhmann 1988, 96 and 1979, 33; Hardin 2002, 72). A probabilistic conception of trust along the lines of a bet, guess or "subjective level of probability" can still be seen as a form of expectation. Indeed, trust is not the same thing as probability; it is rather an expectation of another's actions given a perceived level of probability that the trustee will follow through.

Some define trust as a form of *vulnerability* or *transfer of control* to another person. Elster (2007, 344) defines trust as the lowering of one's guard: one will "*refrain from taking precautions against an interaction partner*, even when the other, because of opportunism or incompetence, could act in a way that might seem to justify precautions." Coleman (1990, 91) sees it as "the

transfer of control over one's actions in a conjoint authority relation" that "must occur at some time before the expected benefits can be realized." This is slightly similar to Baier's (1986, 240) definition as "letting other persons (natural or artificial, such as firms, nations, etc.) take care of something the truster cares about, where such 'caring for' involves some exercise of discretionary powers." On the whole, wording like "refrain[ing] from taking precautions," "transfer of control" or letting other persons "take care of something" are variations on a theme of delegation. Yet even in this sense, trust is not quite the same thing as a transfer of control because transferring control is *action* that is separate from a state of trust. I would argue that for these authors, trust is nevertheless an *expectation* that the trustee will follow through after surrendering discretionary power to her over a task.

If we go by this stock of conceptual development from a wide social science literature, we can define *political* trust as the expectation that political actors or institutions will perform in a way that is consistent with past performance, roles or norms. Luhmann (1979, 26) describes political trust as the expectation of political performance after surrendering control in an election: a political candidate's electoral "success – if it appears at all – does not appear till after the action, while there must be commitment beforehand. This problem of time is bridged by trust, paid ahead of time as an advance on success for a certain time and unless called back…" Trust is based on hypothetical evaluations of actions that can only be observed after the trust has been invested. It is fundamentally "a technique for extending the availability of time and thus rendering possible the choice of delayed gratification, for increasing men's tolerance of uncertainty, for the contrafactual stabilization of expectations" (Dunn 1984, 281). Rosanvallon (2008, 48-49) similarly argues that trust acts as an "invisible institution" or "assumed stock of information," an essential "property of

a relationship between persons or groups, for example, between governors and governed" in which a "politician's reputation becomes his certificate of warranty."

Although this definition seems easy enough to accept in an empirical study, for commentators like Hardin (1999, 25), an expectations-based account of trust does not truly capture what we mean by the term, neither generally nor specifically with regards to political institutions:

> If trust is nothing more than the reasonable factual expectation that another will behave in a relevant manner, than it is nothing more than, say, the trust with which we sometimes inductively assert that the sun will rise tomorrow morning because, after all, it has always risen every morning that we can remember. We may similarly inductively trust some part of the government in this very limited sense because we may simply extrapolate from behavior until this moment to predict future behavior. Of course, this means, among other things, that we can trust some government bodies and agents to continue to act corruptly against our interests, we can trust others to continue to be incompetent to do what they are appointed to do, and we can trust many agents of government to lie to us with regularity about important matters for which they are responsible.

When we trust, Hardin argues, we are not merely expecting consistent behavior from the trustee. We are rather expecting that the trustee will perform a task with the specific *intention* of fulfilling our interest. In other words, our interests will be *encapsulated* in the trustee's actions. Kant's neighbors might expect him to take a morning walk at a certain hour every day as a means of reminding them of the time. But for them to *trust* him to take his walk would mean that he does so expressly in their interest (ibid., 30). Hardin's account transforms the task of conceptualization into theory building about what trust entails. In his view, in order for us to trust, we not only have to hold certain expectations granted some uncertainty about the trustee's subsequent actions, but our trustee must adopt a certain mental state and behave accordingly.

In similar ways, some conceptualizations of trust, particularly in economics, describe not merely what trust is, but how others must respond for it to work. Trust is seen to economize on resources, allowing us to forego monitoring or third party enforcement over the trustee's actions

(Stigler 1961). Like Hardin, Dasgupta (1988, 51) argues that trust entails more than a unilateral expectation or assessment of the trustee's constraints or promises: "You do not trust a person (or an agency) to do something merely because he says he will do it. You trust him only because, knowing what you know of his disposition, his available options and their consequences, his ability and so forth, you expect that he will *choose* to do it." Not only that, but the trustee must be aware that the truster can refuse to cooperate in the future in the case of defection. For Gambetta (1988, 219), "if it were only others who enjoyed freedom while we had no alternative but to depend on them, then for us the problem of trust would not arise; we would hope rather than trust."

On the basis of such accounts of what trust *does* or what is required for trust to *work* in empirical settings, a number of critical voices deem the idea of political trust empirically untenable. Before launching into his critique of the notion of political trust, Hardin (1999, 24) invites the reader to "consider trust in another person, from which we might expect conceptually to be able to generalize to claims about trust in groups, organizations, or institutions." Using his conception of trust as encapsulated interest, Hardin proceeds to argue that "trust" is an inappropriate term to describe one's orientation to political institutions because one has little reason to believe that a large organization (the government, no less) will take her interests into account when performing a "trusted" task. Public officials, Hardin argues, are self-interested and citizens do not usually possess enough information about their motivations or commitments to develop trust in the same way that they do with regards to individuals they know. Offe (1999) writes that "before 'they' [elected officials] act, 'I' have no sufficient evidence to form beliefs about how they are going to act and whether there is sufficient reason to trust. But even after they have acted, what comes to 'my' awareness is at best an aggregate effect, not the specific behaviour of specific individuals that have caused it" (ibid., 56-7).

In Hawthorn's account (1988, 112), four "reality conditions" must be met for trust to sustain cooperation: that "people know what each other's motivations actually are, that they know that they know, that this knowledge is not too expensive to obtain and maintain, and that the outcomes of any course of action are not too difficult and themselves too expensive to determine." He concludes that in this case trust is only possible between friends. If one were to transfer micro-level trust characteristics to more complex social settings, "the only possible society is an aristocracy" in which "simplified, stylized, symbolized" social relations achieve a level of familiarity and reciprocity similar to personal friendship (ibid., 114). Based on fieldwork research on trust relationships in the slums of Accra, Ghana, Hart (1988) also suggests that trust is conceptually relevant only to friendship based on free and voluntary association. Trust must be granted in conditions where reliance on the trustee is freely chosen, something which is not always possible in one's orientation to political institutions.

In short, a number of empirical approaches suggest that it would be inappropriate to describe one's evaluations of political institutions as "trust" on grounds that the relationship between citizens and their government does not meet the criteria necessary for trust to *work* in the same way that it works between individuals. Such critiques would lead us to conclude that political trust is an empty concept simply because it does not refer to any real empirical phenomena. In my view, these critiques are not entirely correct because they conflate what trust *does* or what is required for it to *work* with what trust *is*. However desirable these criteria, it ultimately does not matter whether a citizen has the ability to choose or terminate agreements with political authorities, whether she has intimate knowledge of their motivations or whether they encapsulate her interest while in office. It still makes sense to say that she expects political authorities and institutions to perform in a certain way. When she is asked to rate her trust in government, her response is fully

consistent with an expectations-based account of trust. Auxiliary questions about how trust *works*, or whether it works *well* are empirical, not conceptual in nature. Even if we were directly interested in answering such empirical questions at the present time, we would have no reason to assume that individual-level trust *works* in the same way as institutional trust. Such claims would have to be properly theorized and tested.

Despite the conceptual and empirical limitations of Hardin's approach, he *is* correct in pointing out that we do not merely expect continuity when we evaluate political institutions. When prompted to rate our trust in government, we are probably not thinking, "I trust my government to always be corrupt." We are instead likely rating whether the government's performance is compatible with our notion of what is right or desirable. This is a specific, *normative* conception of trust which depends on citizens' beliefs about how institutions ought to perform. Hardin (2002, 153) himself lands close to this proposition, suggesting that "the question of whether we can reasonably trust institutions reduces to the question of whether institutions can be trustworthy" or, in other words, "reliably expected to fulfill their missions." In this perspective, what we really mean when we say we trust a political institution is that we believe that it is *trustworthy*. Offe (1998, 70) shares this interpretation, arguing that institutional trustworthiness is based on a commonly held understanding in society of what institutions ought to be doing: institutions are

> endowed with a spirit, an ethos, an implicit moral theory, an idée directrice, or a notion of some preferred way of conducting the life of the community…*it is this implied normative meaning of institutions and the moral plausibility I assume it will have for others which allows me to trust those that are involved in the same institutions* – although they are strangers and not personally known to me.

Pettit (1998, 305) narrows this normative conception, equating political trustworthiness with republican virtue: "Every republic has to be one of morals as well as one of laws." Beyond law and regulation, political institutions must be sustained by elected officials' adherence to commonly

shared moral principles and civic duties. Virtuous, trustworthy, or synonymously "legitimate" government depends on "leaders' keeping faith with the citizens who have given them authority to act on the public's behalf" so long as citizens believe that the government will "act in their interests, that its procedures are fair, and that their trust of the state and of others is reciprocated" (Levi 1998, 88).

The idea of political trustworthiness is similar to political *legitimacy*. Indeed, the concept of "political trust" is often seen as a proxy for political legitimacy in the survey-based empirical literature (Anderson and Tverdova 2003; Chang and Chu 2006; Christensen and Laegrid 2005; Hooghe 2011; Kim and Voorhees 2011; Mishler and Rose 2001; Suh et al. 2012). In defining legitimacy, Beetham (2013, 11) argues that "we are making an assessment of the degree of congruence, or lack of it, between a given system of power and the beliefs, values and expectations that provide its justification." Power in this account rests not on one's ability to exercise authority, install proper incentives or enforce sanctions, but on "the degree of others' willingness to cooperate," which "depends on the normative status of the power holder, and on the normative considerations that engage us as moral agents" (ibid., 38). This is a thinner notion of legitimacy than what Pettit and Levi describe in so far as it makes no claims about the necessity of virtue or the performance of civic duties in legitimating political authority. What a republican theorist might consider non-virtuous behavior by a political authority can still be legitimate if congruent with shared understandings of proper behavior in the public.

Despite frequent references to these notions in the empirical political trust literature, political trust is a highly imperfect proxy for political legitimacy or trustworthiness.

Measures of empirical legitimacy are often defined both in terms of individuals' subjective "judgement of [the] appropriateness" of institutional behavior and in terms of their "judgement of

entitlement," or their belief that the institution is entitled to its authority (Jackson et al. 2017, 2). While the former subjective element of legitimacy might be reasonably proxied by a simple political trust question, the latter element is better captured with survey questions assessing compliance with the decisions of the institutions under inquiry. That is, if you believe an institution is entitled to its authority, you will agree to its decisions or rules. One element of legitimacy concerns attitudes while the other concerns actions: "Critically, the empirical study of 'popular' legitimacy addresses antecedents (the inherently value-based set of criteria that citizens use to judge the right to rule) and behavioral consequences—legitimacy, after all, is a reason to act" (ibid.). A comprehensive empirical study of legitimacy will therefore require the researcher to assess both components. Although political trust (broadly conceived as an expectation of future performance) can indicate a normative match between individuals' expectations and institutional reality, *legitimacy* is a more complex empirical phenomenon that cannot be operationally captured by indicators of political trust alone.

In short, a number of interpretations of political trust are possible when we ask someone to rate her trust in government. 1) The broadest of these regards political trust as an expectation of continuity. This expectations-based account encompasses interpretations of trust as a delegation of authority to others or a bet about the probability of a trustee's performance of a task. 2) A narrower, normative conception of trust is not merely an expectation of continuity, but an expectation that political authorities will act in accordance with the truster's wishes. 3) A third way of conceptualizing political trust is as a proxy for political trustworthiness or legitimacy. Some approaches urge that trust is only trust as long as it advances cooperation or conformity to political authority, or that our ability to trust depends on what others do with it.

I argue that we should not conflate empirical approaches about what trust *does* with conceptual ones about what trust *is*. Although political trust does not result in cooperation between citizens and authorities in the same way that it does between individuals, it does not mean that it is a barren concept or that trusting government, as Hardin (2002, 152) argues, is "conceptually and epistemologically impossible for most citizens in large modern societies…" The way we define political trust, I would argue, should remain limited to a normative conception of trust (the second option above) which describes people's expectations of institutions in accordance with their beliefs about how those institutions ought to perform. It is neither a general expectation of continuity nor a proxy for legitimacy. It is "normative" in so far as it relates to what *people* think is desirable performance, not what theorists studying political trust think is desirable performance.

It would seem, however, that a student of political trust can say very little about it without understanding these normative frames of reference. Discovering the criteria people employ in their evaluations of political institutions is an important, but highly contested empirical exercise. Getting at this rich normative content has become the center of an increasingly prominent but unresolved discussion as survey data has expanded around the globe. Although we have steadily increased the territorial scope of our studies on political trust, it seems that we have come to know less and less about what we are truly capturing with our survey questions. At the same time that researchers have begun to develop more sophisticated operationalizations of political trust, we have become less confident about the validity and comparability of the resulting measurements across countries. In the interest of working with a measurement which allows us to draw the most valid comparative inferences about political trust around the world, I now delve into this empirical debate and attempt to suggest how we can balance its conflicting sides.

1.3 How do we measure trust?

If we define political trust as an expectation of political performance in accordance with people's normative beliefs, we invite the question, what are their normative beliefs? What kinds of ideas underpin one's relationship with her government? We do not get a clear answer to this question in the empirical literature on political trust. Beyond the many survey-based studies that view political trust as a proxy for legitimacy, much of the literature circumvents conceptually strict accounts of political trust. Easton's (1975, 436) often-cited definition of political support is a "way in which a person evaluatively orients himself to some object through either his attitudes or his behaviour." Specific support is a form of "response to the authorities" and diffuse support consists of a "reservoir of favourable attitudes or good will that helps members to accept or tolerate outputs to which they are opposed…" (ibid., 437). Nothing is said about which outputs are at stake. Hetherington (1998, 791) also demands little of the concept and reiterates the normative conception of trust I outlined above: political trust is "a basic evaluative orientation toward the government founded on how well the government is operating according to people's normative expectations." At its most basic interpretation, political trust is a form of "mass support for the political system," "faith in the political process," "confidence in political institutions," "popular satisfaction" with public services, "allegiance to the political system" and "approval" of politicians' actions without further conceptual elaboration (Catterberg and Moreno 2005; Cho and Kirwin 2007; Citrin 1974; Clausen et al. 2011; Lavallée 2008; Wroe et al. 2013; Yang and Tang 2010).

Our limited understanding of what constitutes a trustworthy institution for respondents has slowly become the center of some contention in the survey literature. Rose's (1991, 448) now classic text on forms of comparative analysis remains relevant for the present research problem:

"to amass materials without regard to concepts is to produce empirical data that will sink under its own weight, lacking ideas that give it meaning." Overwhelmingly, it appears that this advice has been sidelined, resulting in somewhat of a "going back to the theory" movement. The lack of any "clear account of what is meant by trust in the first place" in the empirical literature says very little, "if anything," about "the merits of one theoretical concept of trust versus the merits of another" (Nannestad 2008, 415-16). Normative accounts about the importance of trust for democracy inappropriately precede proper definition and measurement (Bovens and Wille 2012, 48; Hooghe 2011, 270). Even before the bulk of cross national research on the topic appeared, Weatherford (1992, 152) argued against "measurement-driven research," noting that the debate about the meaning of political trust and legitimacy

> begins with the measures and tries to fit theoretical inferences to them, rather than reverse, and it promotes the question of policies-versus incumbents by construing legitimacy in terms of public approval for governmental outputs, rather than the more theoretically central issues of how citizens evaluate the system's procedural efficiency or distributive fairness.

This intuition has returned more forcefully in the past decade. Thicker definitions of trust, it is argued, are necessary for subsequent measurement and empirical analysis. Many recent proposals follow conceptions of trust closer to Pettit's idea of republican virtue, attempting to distill it into clear measures of trustworthiness. Articulating a theoretically sound concept of trust for empirical applications, for instance, Levi and Stoker (2000, 498) recommend Miller and Listhaug's (1990) definition of trust as something that

> reflects evaluations of whether or not political authorities and institutions are performing in accordance with normative expectations held by the public. Citizen expectations of how government should operate include, among other criteria, that it be fair, equitable, honest, efficient, and responsive to society's needs. In brief, an expression of trust in government (or synonymously political confidence and support) is a summary judgment that the system is responsive and will do what is right even in the absence of constant scrutiny.

Alternatively, Levi and Stoker (2000, 499) see a second approach to defining trustworthiness as a function of encapsulated trust in Hardin's conception (how much one expects the institution to act in her interests). To apply this empirically in survey research,

> one would first stipulate an *objective* account of interests, and then design survey questions that asked people whether they believed an actor was serving, or not obstructing, those interests. If, for example, one stipulated that people want (a) to be told the truth, (b) to be autonomous, (c) to be treated fairly, (d) to accumulate wealth, and (e) to live free from fear, then one would need to gauge whether a given political actor or institution was seen as protecting or harming those interests (my italics).

Similarly, Kim's (2005, 622) model of institutional trustworthiness contains five main criteria which might be considered an "objective account" for empirical testing:

1) Credible commitments ("Do institutional actors honor their commitments?")
2) Benevolence ("Do they want to do good to citizens?")
3) Honesty ("Are representatives of institutions telling the truth?")
4) Competency ("Do institutional office holders have the necessary knowledge and skills?")
5) Fairness ("To what extent are they dealing with everybody in an equal manner?")

Along slightly different lines, Fisher et al. (2010) argue that different political institutions elicit different types of trust judgments which can be operationalized along strategic, moral and deliberative dimensions. Strategic trust is a rational judgment about the trustworthiness of individual actors, moral trust refers to the truster's belief that most others can be trusted (regardless of individual characteristics), and deliberative trust is based on trusted parties' prioritization of common consensus and mutually beneficial outcomes above individual interests. To be considered trustworthy, political structures "*should* be aimed at full, equal, informed, and un-coerced participation in the rule- and regulation-making process by individuals who are guaranteed the freedoms and opportunities necessary to achieve this end" (ibid., 15, my italics). Here, legitimacy

requires channels for and commitment to rational discourse. At the core of the argument, standard trust in government survey questions fail to incorporate this conceptual multidimensionality.

Although it might be useful to flesh out what kinds of criteria people have in mind when evaluating political institutions, the above attempts do not accomplish this because they present theories about what trust *ought* to do or what it *should* entail rather than what it actually entails. Despite their differences, each approach emphasizes a theoretically driven approach to empirical analysis: prior to conducting survey-based research, one must specify that citizens expect X, Y and Z of their government and their government must in turn exhibit X, Y, Z characteristics or perform X, Y, or Z services. These specifications are a-contextual, universally applicable and even *objective*. The thrust of this "going back to the theory" argument is that after years of neglectful thinking about concepts, empirical researchers must re-endow measures of political trust with meaning before engaging in causal or normative inference.

I would argue that identifying the criteria of trust is an important and necessary challenge, but doing so requires empirical work. While we as researchers might believe that people wish to accumulate wealth or to be told the truth when evaluating political institutions, these are not facts but hypotheses in need of testing. To take Fisher et al.'s conceptualization of legitimacy as an example, a commitment to rational discourse might be a desirable element of political legitimacy in theory, but its absence in authoritarian regimes might lead us to conclude that such governments are altogether illegitimate. If we consider anthropological work on political legitimacy in China (Li 2004) and Azerbaijan (Barrett 2015), we will find that even authoritarian regimes elicit compliance and genuine admiration from ordinary people for reasons that have nothing to do with their capacity for rational discourse about political life. The validity of our empirical measures requires that they capture what they mean to measure in the real world. Conceptual complexity

alone does not qualify a measurement model for empirical applications. If the model is untested for content validity in the empirical context where it will be subsequently analyzed, *no amount* of a-priori theoretical richness can justify its empirical usefulness. It should come as no surprise, indeed, that adding complexity can do more harm than good, especially when the measures are meant to be compared in heterogeneous environments.

Gilley's (2006) widely cited attempt to create a generalizable measurement model of legitimacy in 72 countries demonstrates some important limitations of "going back to the theory" to aid empirical work. I have argued earlier that measures of political trust are *not* proxies for political legitimacy despite popular arguments for this interpretation. Nevertheless, Gilley's study is an important example of the problems of measurement validity and equivalence that one can easily encounter when measuring political trust in any of the ways recommended by the above authors. I will use his study merely as a means of illuminating the tension between theory and empirics that we often face when doing comparative work.

Gilley's proposed measurement of political legitimacy is based on three conceptual subtypes of legitimacy including citizens' views of legality (measuring the extent to which political institutions exercise power in a way that accords with citizens' interpretation of rules and laws), views of justification (measuring the "moral congruence" of actions by political institutions and citizen expectations) and acts of consent (observable behavior by citizens demonstrating the state's right to exercise its authority). For all three subtypes, Gilley notes that "we are concerned with rightfulness 'as believed' by citizens rather than rightfulness 'as claimed' by rulers" (ibid., 502). As such, the measurement depends on citizens' normative evaluations of political institutions. Three indicators are chosen for each subtype and the nine total indicators are aggregated into a 1-10 scale.

This choice of indicators makes for a troublesome cross-country comparison. "Views of justification" are measured in part by World Values Survey (WVS) questions about people's satisfaction with democratic development, evaluations of the current political system and satisfaction with the operation of democracy in their country. Voter turnout is used as a measure of "acts of consent." Off the bat, these measures assume the presence of democratic institutions and functional electoral systems in 72 countries.

Mishler and Rose (2001) demonstrate that assessments of legitimacy or system support based on the above WVS questions on democracy are markedly different from those based on a question simply gaging evaluations of the political system (without reference to the sort of political system). Based on the WVS question on democratic satisfaction in the 1995-1997 wave, Mishler and Rose (2001, 306) report the fascinating discrepancy that 25 percent of United States respondents are satisfied with democracy, while that number is 77 percent in Azerbaijan (where, among various abuses of political rights, the president gained full control over the government, legislature and judiciary in the 1995 constitution). Gilley is somewhat perplexed that his aggregated legitimacy index ranks Azerbaijan as the tenth most "legitimate" country out of 72, placing it between the United States and Germany. China also ranks impressively at 13, above established democracies like Britain (18), New Zealand (23) and France (33). Gilley calls Azerbaijan a "failed post-Soviet democracy," a title which likely reflects an idealized view of post-communist transitions as heading towards democracy rather than the more realistic interpretation of authoritarian consolidation with cosmetic democratic features (Levitsky and Way 2015). Whether or not this influenced Gilley's decision to use measures of satisfaction with democracy, we can see that such "idealist" measures (in the words of Mishler and Rose) lack empirical

referents in several countries in his sample, which almost certainly undermines the measurement validity he wishes to establish.

This questionable validity of legitimacy measures in separate countries makes them difficult to compare. Gilley expressly argues that legitimacy is "normative by conceptual definition" (ibid., 502). At the same time, his universal legitimacy scale divorces itself from that local normative context even though it inherently depends on it. This disconnect between measure and reality obscures the offered cross-country comparison. If we take Beetham's definition of legitimacy seriously, it would be necessary to make explicit the norms on which citizens' expectations of political institutions are built. Measures of satisfaction with democracy or violent civil protests do not capture those norms.

Bo Rothstein's (2009) work on political legitimacy is another example of the possible dangers of making theoretical assumptions in measurement building. Rothstein argues that the mere possibility of political representation (i.e. only holding elections) is insufficient to make the exercise of power legitimate. Instead, political legitimacy depends on the "quality of government," which he defines as "procedural fairness based on impartiality." It therefore follows that the "absence of corruption, discrimination, and similar violations of the principle of impartiality in exercising political power" will serve to "create political legitimacy" (ibid., 325). Linde (2011) follows Rothstein in his study of political legitimacy, arguing that "to be considered fair, the public administration must treat individuals impartially in the allocation of goods and services," noting that "although not a coherent body of research, most studies engaged with the 'quality of government' have one thing in common: They regard corruption as one of the most, if not *the* most, serious challenge to high quality government" (ibid., 413).

The problem with this approach is that it is based on what scholars, not survey respondents, believe to be fair institutional performance. While citing one paper which demonstrates that corruption perceptions corrode political trust in Latin America as evidence, Rothstein does not convince us that we would observe losses of legitimacy given such partiality. Indeed, there are plenty of empirical cases in which the counterfactual is invalid: administrative corruption, the forceful payment of bribes, the necessity of using networks to obtain public services and the general misuse of public office for private gain fail to corrode political legitimacy and incumbent approval across the regime spectrum (Chang 2013; Golden 2006; Manzetti and Wilson 2006; Rundquist et al. 1977; Fernández-Vázquez et al. 2014; Winters and Weitz-Shapiro 2013; Zechmeister and Zizumbo-Colunga 2013). If we were to take Rothstein's definition of legitimacy seriously, we might conclude off the bat that governments in these empirical cases are not legitimate without needing to consult how citizens actually relate to their political institutions. Measures of legitimacy incorporating Rothstein's conception of procedural fairness would simply not correspond to the reality they purport to describe. Although from a theoretical standpoint one might believe that people do not favor corruption, this is an empirical hypothesis that must be tested. It cannot be assumed to be part of the criteria people use when evaluating political institutions. And if it *is* part of what goes into people's institutional trust and conformity to political authority, we cannot assume that people respond to corruption in the negative way *we* as investigators think they should.

It follows that Rothstein's (2009, 326) argument that "political legitimacy ought to be the ultimate goal for any system of governance" is normatively flawed. Some political systems gain support on the basis of less liberal conceptions of fairness than what he suggests. If it turns out that citizens regularly benefit from corruption and reward politicians for sharing spoils, should the

political legitimacy that results from such practices be the ultimate goal for that system of governance? Indeed, Rothstein's normative prescription is premature without an empirical inquiry into the underpinnings of political trust and legitimacy.

But it is worth noting that Rothstein is not alone in his normative suggestion. The urgency to maximize political trust or legitimacy prior to empirical inquiry has a long and thriving legacy dating back to the "crisis" of democracy literature from the 1960s and 70s. Easton (1965), Luhmann (1979) and Parsons (1961) feature prominently in works cited lists of many recent empirical investigations of political trust, where authors anticipate scenarios in which states respond to low trust levels with ratcheting levels of coercion. In their study of sources of support for European integration, for instance, Arnold et al. (2012, 3) start their paper with "Trust in political institutions is one of the key elements allowing representative democracies to work." Yang and Tang's (2010, 415) first sentence in their study of the sources of institutional trust in China is similar: "Trust in political institutions is important for the successful functioning of democracy." Describing low levels of trust across post-communist societies in Eastern Europe, Lovell (2001, 35) suggests that "Enhancing trust is a prime responsibility of leadership in postcommunism – it is, in an importance sense, a *definition* of leadership…Trust can be built by exemplary, but not necessarily heroic, behavior, especially on the part of publicly prominent role models such as politicians." Bowser (2011, 17) echoes the approach: "If the countries of the FSU [Former Soviet Union] are to proceed to genuinely representative and accountable democratic regimes they must seek to increase public support for the state."

Such statements are often made in introductions as justifications for studying political trust. Yet political trust can be just as interesting for its role in bolstering authoritarian and kleptocratic regimes, and for its potentially harmful place in world affairs. Although it is certainly *plausible*

that greater political trust is a good thing for democracy or political stability, we have reason to be suspicious about accounts which claim that political trust is equally important or beneficial in all empirical settings. Scholars' sophisticated accounts of trust tend to invade the difficult job of understanding what *actually* underpins that trustworthiness for people on the ground and introduces the danger of making misleading claims about its substantive desirability. The normative frame invades the positive, much to the detriment of our understanding of how governments retain popularity in the presence of corruption, extortion, repression and other forms of institutional dysfunction that undermine citizens' wellbeing and ability to lead a good life.

Methodologically, uncovering local knowledge about what constitutes institutional trustworthiness or legitimacy can, in some views, prove to be incompatible with large N comparative research designs. My argument might be broadly interpreted as consistent with Hayek's (1952, 51) critique of what he calls *scientism*, an attempt to reproduce natural science methodology in the social sciences:

> Connected with the tendency to treat the objects of human activity in terms of their "real" attributes instead of as what they appear to the acting people is the propensity to conceive of the student of society as endowed with a kind of super-mind, with some sort of absolute knowledge, which makes it unnecessary for him to start from what is known by the people whose actions he studies.

Hayek's critique is perhaps not perfectly transferable into the current discussion because he was specifically addressing methodological tendencies within the economics profession of the twentieth century to reduce social science data to aggregate statistics, static quantities or precise predictions. Nevertheless, I would argue that his broad message about the importance of understanding the world as it appears to human beings is relevant to modern day large N public opinion research. Perhaps most interestingly, this useful methodological warning comes from an economist, even though resistance to the "scientistic" attitude, particularly with regards to political

trust and legitimacy, is most forceful in anthropology and critical theory, fields which emphasize the importance of the discursive construction of social phenomena.

In his now classic ethnographic work on discourses of corruption and the "imagined state," Gupta (1995, 393) calls for "vigilance toward the imperialism of the Western conceptual apparatus." He argues that instead of premising empirical analysis on concepts of state and civil society "that were forged on the anvil of European history," one must first understand the historically specific discursive processes that make up the world view of the studied population. Complex processes that mediate the social construction of any grouping or class make it far from obvious that ideas like "civil society," the "state" or "public" and "private" can be easily dichotomized or quantified. A comparative analysis of "state" legitimacy rests on the crude assumption that the state is a "cohesive and unitary whole" rather than a historically and culturally specific idea (ibid., 392). Moreover, processes of constructing reality do not fit neatly into the boundaries of a nation-state, making the danger of general quantitative measurement more profound: not only are survey comparativists attempting to measure social phenomena which by the nature of their social construction do not lend themselves to neat categorization, but any preliminary categories will not correspond to cross-national boundaries which comparativists are privy to choose for large N comparison.

This perspective rests at the opposite end of the methodological debate on measurement. While authors like Levi and Stoker recommend listing specific elements of institutional trustworthiness in our accounts of political trust, authors like Gupta might be more likely to argue that no amount of conceptual sophistication intended for universal large N applications will reflect the reality it means to capture. Przeworski and Teune (1966, 553) discussed this dilemma at some length five decades ago. For scholars favorable to Gupta's constructivist approach, "abstracting

particular 'traits' from their particular structural-functional setting is not possible and cross-national measurement cannot have validity" (ibid.). According to conceptual approaches in some of the political trust literature, "single, identical indicators of various cultural traits…have cross-national validity and meaning, and thus comparisons can be made in terms of these single standards, external to each of the cultures involved..." (ibid.). While it is impossible to fully satisfy both ideals, I argue that it is important to find balance between them.

In seeking this balance, I do not believe that the core premises in the divide between constructivists and positivists can be reconciled. By adhering to a position that social reality cannot be understood outside of its discursive construction, it becomes impossible to make generalizable inferences about complex social phenomena from a sample to a wider population, where other discourses thrive and form different understandings of the studied subject. I concede that objective comparisons of social phenomena *are* possible, however, and that balance should be sought between universal and context-specific elements of measurement. A highly universal and comparable set of measures across countries will likely say very little about what political trust means separately in each country. A highly detailed concept of trust which captures local knowledge will be difficult to make universal and comparable across countries. Although all measurement models will be highly imperfect in the way they correspond to reality, some will be more useful than others in facilitating meaningful inference about how a concept operates in different environments.

Which measurement approaches are more useful? I do not believe there are hard and fast rules, but would follow Mishler and Rose (2001) in advocating for using realist over idealist measures in the comparative study of political trust. Realist survey questions, in their view, "avoid abstract, ambiguous, and idealistic labels such as democracy, asking citizens to evaluate regimes

as they have personally experienced them" (ibid., 307). At the time that they were writing, public opinion research was rapidly spreading around the world. Mishler and Rose were pushing back against a tradition of measuring political support in terms of support or satisfaction with democracy, measures which were particularly lacking in meaning for survey respondents in non-democracies. To help ameliorate the problem, they proposed measuring people's evaluations of current institutional performance relative to the past. I believe that even this retrospective evaluation can create problems for cross-national comparability because different historical legacies will add noise to current evaluations, particularly in countries which have experienced political transitions or upheavals in previous years. But the authors' general recommendation for realist measurement is correct. The less assumptions about people's normative considerations are attached to measurements of political trust, the better. Contrary to advocates of "going back to the theory," I would argue that standard, Likert-scale questions about the extent of one's trust in government might not be *ideal*, but at least they are not *idealistic*, and they *can* be useful for cross-national comparison. This is not to abandon sophisticated theories about the foundations of trust, but to urge that they be tested.

In my view, the drive to fill the gap between concepts and measures with more sophisticated concepts has mistaken an empirical problem for a conceptual one. Levi and Stoker (2000, 500) argued that macro-level political trust research "requires, first, establishing the attributes of a trustworthy government and of political actors" while micro-level researchers, "rather than leaving open the question of what figures into trust judgments, must stipulate the attributes that, according to their definition, renders an actor trustworthy, and they must tap people's perceptions of these attributes." But in doing so, researchers are prone to produce idealist measures which lack empirical validity and cross-national equivalence. What constitutes

institutional trustworthiness for people is ultimately an empirical question, not a conceptual one. While inadequate attention to concept building can cause data to sink under their own weight, as Rose (1991) argues, it is also true that the failure to account for analytical decisions in concept building can lead researchers to erect "theoretical skyscrapers on foundations of empirical jello" (Schriesheim et al. 2001, cited in Antonakis 2010, 1114).

1.4 Discussion

When we ask someone to rate her trust in government, we are measuring her expectation that political authorities will perform in accordance with her desires and normative understandings of good performance. We cannot ascertain the content of these understandings without empirical inquiry. Theories about what constitutes trustworthiness to citizens, as well as the causes and consequences of political trust can only be discovered through a concerted application of qualitative and quantitative methodologies. Unfortunately, many quantitative empirical approaches depend on limited survey data. In an ideal scenario, we could design survey questions which inquire into the precise expectations citizens have of their institutions as Levi and Stoker suggest, and test 1) whether people do, for example, wish to accumulate wealth or to be told the truth, and 2) whether these conceptions are similar across diverse populations. Many surveys, however, lack such detailed indictors, leaving us with standard Likert-scale trust in government survey questions. I have argued that these measures are acceptable in so far as they make no assumptions about their normative empirical content. Using them comes at the expense of obtaining rich local knowledge about political trustworthiness in different settings, but it does not preclude meaningful comparative work.

As I show in the next chapter, responses to standard trust in government survey questions can tell us a great deal about how societies under different regime conditions relate differently to different political institutions. In chapters three and four, I challenge Rothstein's conception of political legitimacy as procedural fairness by investigating cases in the former Soviet space where governments retain high levels of political trust despite rampant corruption. In studying this link, it is impossible (and not my motive) to identify all possible normative contributions to political trust, but it is possible to identify conditions under which people support political institutions for self-interested reasons, and when other, possibly more principled expectations and notions of political accountability come into play. And although the scope of this dissertation is limited in its exploration of political legitimacy, I discuss the relationship between incumbent support and people's willingness to challenge their government in chapter five.

2 Chapter 2. Can we Trust Measures of Political Trust? Assessing Measurement Equivalence in Diverse Regime Types

2.1 Introduction[1]

Since the 1960s and 70s, theorists have claimed that political trust is crucial to the function of democracy and political order. Rosanvallon (2008, 48-49) describes trust as an "invisible institution" or "assumed stock of information", an essential "property of a relationship between…governors and governed" in which a "politician's reputation becomes his certificate of warranty." Trust in political institutions allows elected officials to provision public goods to their constituents without resorting to repression or coercion (Parsons 1961, 53; Luhmann 1979, 56). Declines in political trust across advanced democracies in the postwar era have been interpreted as a deterioration of state legitimacy (Easton 1975; Luhmann 1979) and even a "crisis of democracy" (Huntington, Crozier and Watanuki 1975). Recent research shows that low political trust levels are associated with tax fraud and low compliance with the law (Hooghe and Marien 2010) as well as low generalized trust and social capital (Rothstein 2003; Schyns and Koop 2010).

Despite a long standing consensus about the importance of political trust, there is little consensus about its appropriate definition or measurement in cross-national research. Researchers continue to rely on sum scores or averages of standard "trust in government" survey questions without fully understanding what the concept means, or whether these measures tap into comparable ideas across the countries in their sample. Precisely because of the normative and subjective content of political trust, what constitutes a trustworthy institution is unlikely to be the

[1] Supplementary materials for this chapter are available in Appendix A. An abridged version of this chapter was published in *Social Indicators Research* in 2016: https://link.springer.com/article/10.1007/s11205-016-1400-8

same for citizens in different cultural and regime contexts. Moreover, cross-national research in more diverse environments enhances the potential for measurement error resulting from the data collection process with possibly detrimental consequences for regression analysis. Prior to comparing the means or correlates of political trust survey indicators, it is important to check that the indicators deliver similar and comparable understandings of political trust across populations. Little progress has been made in this direction even though our ability to accurately theorize about the causes and consequences of political trust depends on these empirical considerations.

To account for this weakness in the comparative literature, I aim to answer two questions about the measurement of political trust in 35 European and former Soviet countries using the 2010 Life in Transition II Survey (LITS II). First, do standard "trust in government" survey indicators represent a single, comprehensive attitude of political trust? Second, are different measurement models of political trust equivalent in all countries? In other words, do measures of political trust travel successfully on the survey instrument across borders? I will investigate these questions using multiple group confirmatory factor analysis (Jöreskog 1971). Unlike most studies of the measurement equivalence of political trust, the results of this study will help us determine our ability to compare the means and correlates of the construct across both democracies and non-democracies.

2.2 Empirical Considerations

Data limitations are inevitable in the measurement of political trust. Most surveys do not contain indicators which capture elements of institutional trustworthiness for a given population. As a result, scholars typically produce sum scores or averages of survey indicators measuring trust in a variety of political institutions on a Likert scale (from strong distrust to strong trust). Usually,

little to no rationale accompanies these item choices. To consider just a few examples from highly cited studies, Mishler and Rose (2001) examine the sources of political trust in ten post-communist societies surveyed in the New Democracies Barometer by averaging trust in the parliament, prime minister or president, courts, police, parties and military. Chang and Chu (2006) and Chang (2013) use the East Asian Barometer to estimate the effect of corruption on political trust in six Asian countries by averaging trust in the president/prime minister, courts, national government, political parties, parliament, civil service, military, police and local government. To assess the importance of political capacity for political trust in environments with different levels of violence, Hutchison and Johnson (2011) construct an additive index of trust in the executive, courts, police, armed forces, electoral commissions and government-run media for 16 countries surveyed in the Afrobarometer. Clausen et al. (2011) use the Gallup World Poll to study political trust and corruption in 103 countries, obtaining an index of confidence in public institutions by summing responses to a question on confidence in the military, judicial system and courts, national government and honesty of elections. Similar measurement approaches are taken in cross-national research projects on political trust in Latin America (Seligson 2002; Stoyan et al. 2014), Asia (Wong et al. 2011), Sub-Saharan Africa (Cho and Kirwin 2007; Lavallée et al. 2008) and other global samples (Catterberg and Moreno 2005; Hakhverdian and Mayne 2012).

This prolific "kitchen sink" measurement approach is a problem if our goal is to draw meaningful inferences about political trust across diverse societies. Comparing the correlates of averaged or summed indicators across countries assumes that these indicators are 1) reliable, unidimensional measures of political trust in each country and 2) mean the same thing to respondents in each country. Could we assume, for instance, that citizens on the democratic and autocratic parts of the regime spectrum have similar understandings of what it means for an

election to be honest? Can we be sure that fear to report true beliefs about the executive is not biasing the responses of a person affected by a violent civil war? Do citizens of East Asian countries really evaluate the president and police in similar ways? Perhaps petty corruption might reduce trust in the police but not in a charismatic president.

While testing for the validity and equivalence of empirical measures is common practice in psychology and management studies, it is less recognized in political science (Adcock and Collier 2001, 536; Ansolabehere et al. 2008, 228). Measurement error in survey research can result from inherently different understandings of survey questions across different populations, as well as from method effects specific to the survey instrument. Survey implementation, translation, and question order can influence nonresponse patterns, uses of extreme response categories and socially desirable responses by population (Davidov et al. 2014, 59-62; Podsakoff et al. 2012, 544). In applied research, cross-cultural comparisons of attitudes toward democracy, levels of postmaterialism and left-right political ideology, for instance, do not pass the test of measurement equivalence (Alemán and Woods 2015; Davidov et al. 2014). Delhey et al. (2011) find significant cross-national variation in the way respondents interpret "generalized trust" in the World Values Survey by estimating their "trust radius," or the width of one's notion of trust in "most" people. They find that the trust radius is much smaller for people in countries with Confucian influence than for those in countries with a Protestant heritage and modern economy, noting that such findings "throw sufficient doubt on the cross-national validity of the standard trust question" (ibid., 793).

For the most part, these examples of non-equivalence are the result of *construct* biases, or differences in people's understandings of the survey question at the level of the construct. In other words, responses to a question intended to measure left-right orientation, democratic preference,

postmaterialism or social trust reflect different notions of what these constructs mean in different populations. This notional variation poses problems for measurement because individuals in different populations with the same substantive position on the construct will end up scoring different items on the scale. Individuals in China and the US may score systematically differently on questions of generalized trust not because one population is actually less trusting than the other, but because one population has a different understanding of what counts as the generalized "other." It is in this sense that the survey instrument becomes cross-culturally *incomparable* or *inequivalent*.

Measurement equivalence testing procedures are only recently appearing in survey-based political trust research. Hooghe (2011) uses factor analysis to show that British citizens do not distinguish between MPs, governing parties, opposition parties or the head of state regardless of political sophistication or education. Suh et al. (2012, 516) demonstrate in a latent class analysis that trust in government is part of a broader set of attitudes towards public and private institutions like companies and civil associations in South Korea. A number of studies implement a multiple group confirmatory factor analysis (MGCFA) of political trust models in the European Social Survey (ESS), all finding relatively strong evidence for the equivalence of political trust across subsets of countries and time points. The authors' choices of indicators, however, are not entirely theoretically consistent.

Allum et al. (2011, 42), for instance, use only trust in the parliament and politicians, arguing that a prior confirmatory factor analysis (CFA) showed that trust in the legal system, police, European parliament and UN constituted a separate dimension of trust. Coromina and Davidov (2013, 41), however, choose trust in the parliament, legal system and politicians using the same survey. They do not justify this precise combination of indicators even though they acknowledge

that legal institutions like courts are often conceptualized separately from strictly political institutions because they are meant to be impartial and focused on enforcing the rule of law (Jackson et al. 2011; Linde 2012; Rothstein and Stolle 2008). Marien (2011) chooses the broadest set of indicators from the ESS, including trust in the parliament, politicians, political parties, legal system and police, detecting an error correlation between trust in the police and legal system in Eastern Europe. This finds some confirmation in Schaap and Scheepers' (2014) assessment of the measurement equivalence of trust in police in 27 European countries. André (2014) also uses a broad range of indicators in the ESS to test for the equivalence of political trust between EU natives and migrants, but introduces three correlated errors to illustrate the multidimensionality of the construct as distinctively political (measured by trust in politicians, parliament and political parties), order/neutral (trust in the legal system and police) and international (trust in the EU parliament and United Nations).

Although these papers have different theoretical purposes, it is striking how many different models of political trust can fit mostly the same data in a single region of the world. A cross-regime survey will likely invite even more variation in indicator selection and increase the potential for measurement error across countries. There is, however, no substitute for conscientious measurement modeling in regression analysis. Feeding unreliable and non-equivalent measures into a pooled regression can undermine the validity of substantive results. Reeskens and Hooghe (2008, 527) and Coromina and Davidov (2013, 48) show that using country means without accounting for measurement error and equivalence results in incorrect country rankings on social and political trust. Bertrand and Mullainathan (2001, 70) go as far as to argue that "subjective variables cannot reasonably be used as dependent variables, given that the measurement error likely correlates in a very causal way with the explanatory variables." If a sum score of political

trust is used as a dependent variable, for example, predictors might not ultimately be explaining variation in political trust scores, but rather the way those scores are over or under-reported. Westfall and Yarkoni (2016, 12) and Van der Veld and Saris (2009, 38) demonstrate that multiple regression capitalizes on measurement error by incorrectly apportioning the amount of explained variance in the dependent variable between different error-laden predictors. Both authors recommend structural equation modeling (SEM) to control for measurement error and thus more accurately determine each predictor's effect on the outcome.

Ultimately, though political trust is considered an important object of study, it currently rests on a weak theoretical and empirical foundation. Neglecting the criteria for empirical measurement can undermine our ability to draw meaningful and accurate inferences about substantive theories using regression analysis. While this issue is gaining traction in political trust research, most tests of measurement equivalence remain limited to Europe and specifically to the European Social Survey. Techniques like MGCFA have not yet enriched measurement modeling in developing and authoritarian countries where survey research has proliferated in the last decade. To help overcome this weakness, I will put to test the ability of different measurement models of political trust to meet the requirements of validity and equivalence across different cultures and regime types.

2.3 Case Selection and Data

Considerable region-specific research on political trust outside of Europe and the United States has covered parts of Asia, Africa, Latin America, and selections of contemporary and developing democracies. Political trust in parts of the former Soviet space has received some empirical attention (Luhiste 2005; Mishler and Rose 1997, 2001; Wallace and Latcheva 2006),

although these regional samples have neglected countries in the Southern Caucasus and Central Asia mainly due to a lack of data.

In this study I use the Life in Transition Survey II (LITS II) produced by the European Bank for Reconstruction and Development (EBRD) and the World Bank. The cross-sectional sample from late 2010 surveys almost 39,000 households in thirty-five countries to assess public attitudes on a range of social, political and economic variables. This sample contains all former Soviet countries, the Balkans, Eastern and Central Europe, and some of Western Europe.[2] Two – stage clustered, stratified sampling was employed across regions in each country.[3] 38,379 response observations (with at least 895 per country) are used in this analysis due to missing values on all variables in 485 observations. Respondents were asked "To what extent do you trust the following institutions?" (including the presidency/monarchy, government/cabinet of ministers, regional government, local government, parliament, courts, political parties, armed forces, police) using a 1-5 Likert scale (from complete distrust to complete trust).

Helpfully, this survey includes Central Asia and the Southern Caucasus, allowing for systematic comparison between significantly different cultures and regime types. The inclusion of these rarely-explored regions introduces striking variation on trust perceptions into the sample. Looking only at the single 'trust in government' indicator without controlling for measurement error, the most authoritarian countries appear to be the most trusting of government with the exception of Sweden (Uzbekistan exhibiting the most trust, followed by Tajikistan, Sweden,

[2] Here is the full list of countries with each sample size in parentheses: Albania (1029), Armenia (948), Azerbaijan (988), Belarus (895), Bosnia (1075), Bulgaria (1007), Croatia (997), Czech Republic (1006), Estonia (989), France (1008), Georgia (959), Germany (1032), Great Britain (1447), Hungary (1028), Italy (1046), Kazakhstan (943), Kyrgyzstan (992), Latvia (995), Lithuania (1003), Macedonia (1058), Moldova (1023), Mongolia (980), Poland (1587), Romania (1068), Russia (1549), Serbia (1506), Slovakia (995), Slovenia (984), Sweden (899), Tajikistan (996), Turkey (996), Ukraine (1547), Uzbekistan (1417), Kosovo (1081), Montenegro (966).

[3] See the full methodological report on the EBRD website: http://www.ebrd.com/news/publications/special-reports/life-in-transition-survey-ii.html

Azerbaijan, Kazakhstan, Turkey, Belarus, Georgia, Montenegro and Russia). On this basis it could be argued that subjecting these indicators to cross-national comparison inappropriately assumes that the institutions in question have sufficiently similar roles and functions. After all, parliaments and political parties can be considered fundamentally dysfunctional in autocracies like Uzbekistan, and thus incomparable to the same institutions in Sweden.

While it is true that a host of political institutions are functionally dissimilar in democracies and autocracies, the parameter on which comparisons are drawn in the present case (as in much of the political trust research) is based on perceptions rather than observable attributes of institutions. In testing the validity of a perception-based measure, we must ensure that we are working with approximately similar associations of political trust among respondents in multiple locations. If, say, the parliament proves to be different enough in two countries so as to inspire inherently different understandings of its purposes and functions, this deviation can be detected by measurement equivalence testing. Likewise, biases in response arising from fear to reveal genuine opinions or misinterpretations based on faulty question translations or interview techniques may also lead to statistical nonequivalence. A comparison between countries on the indicator would be deemed un-interpretable in these cases. If, however, despite significant differences in institutional functionality attitudes across countries refer to the same approximate idea, we can proceed with comparisons on the perception-based measures even in diverse regime contexts.

2.4 Analytical Strategy

I use Multiple Group Confirmatory Factor Analysis (MGCFA), a powerful statistical tool in the family of structural equation modeling commonly used to assess the measurement equivalence of a latent construct across populations. MGCFA is typically employed after valid

measurement models have been specified in all groups (in this case, countries) either via exploratory factor analysis (EFA) or a strong theoretical foundation, and tested for appropriate "goodness of fit" to survey response data via confirmatory factor analysis (CFA). More specifically, the purpose of EFA is to explore inter-correlations among a set of indicators to generate the smallest number of unique factors that can best explain these correlations (Brown 2006, 20). In CFA, rather than simply exploring data for sets of patterns, the researcher uses theoretical reasoning to make a-priori specifications of the measurement model, constraining specific indicators to load on specific factors. This procedure tests the validity of the researcher's measurement model by showing how much the model specification adheres to covariance patterns in the survey data.

Measurement equivalence is used interchangeably with the statistical term "invariance," which refers to "whether or not, under different conditions of observing and studying phenomena, measurement operations yield measures of the same attribute" (Horn and McArdle 1992, 117; Steenkamp and Baumgartner 1998, 78). Conventionally, three stages of invariance must be achieved before the comparison of means can occur. When testing for *configural invariance*, we want to understand whether the same survey indictors measure the same latent construct in all groups. Reaching this level means that the same basic meaning and structure of political trust exists in all countries. *Metric invariance* refers to the equality of factor loadings on the construct across groups. A unit change in the latent factor of political trust will affect scores on political trust survey questions by the same magnitude across countries. Reaching this level of invariance is sufficient for meaningful cross-cultural comparisons of covariances and unstandardized regression coefficients. That is, we can be confident that changes in political trust scores arise from real differences in the underlying construct rather than nuisance variables or method effects (Byrne et

al. 1989). Finally, *scalar invariance* refers to the equality of intercepts across groups. In this case, differences in indicator means result from differences in latent factor means (Steenkamp and Baumgartner 1998, 80). Although this level of invariance is typically required to meaningfully compare factor means across groups, *partial scalar invariance* is generally considered sufficient if at least two indicators per factor have invariant loadings and intercepts in each group (Byrne et al. 1989; Byrne 2012, 198; Brown 2006, 81-2). While respondents might understand survey questions similarly (given metric invariance), it may still be problematic to compare means if the model fails to achieve partial scalar invariance.

I follow the literature in assuming that political trust indicators *reflect* a broader attitude toward political institutions; they do not *generate* it like education, income and occupation generate the concept of socioeconomic status. Indicators like trust in the parliament, political parties and prime minister have been shown to be highly correlated and interchangeable in the CFA and MGCFA literature, which is the opposite of what one would expect of a formative or 'generating' approach to index construction. In the latter approach, items should have a distinct influence on the measured construct in a way that it would lose substantive meaning without each item. High inter-correlations among items would signal redundancy and multicollinearity rather than reliable internal consistency (Diamantopoulos and Siguaw 2006, 267). It would be hard to argue that any single political trust indicator adds a distinct or essential contribution to a person's broader political orientation. More realistically, political culture inside a country affects how one relates to a number of political institutions (Marien 2012, 17).

Perhaps most importantly, trust items in a single index can co-vary in different ways in different populations. As previously noted, notional differences or method effects can lead respondents to mark different responses on the survey scale even when they otherwise have the

same position. At the level of the construct, some items in the index may form part of what respondents believe constitutes political trust in some countries, but not in others. A formative approach to index construction would also prevent us from detecting this potential *multidimensionality* of political trust across populations, leading to possibly erroneous conclusions about the cross-cultural comparability of the index. For both these reasons, I take a reflective approach to measurement in this investigation: the arrow of influence in the measurement models below moves from the latent, unobserved dimension of political trust to one's position on the observed indicators.

2.5 Constructing Four Measurement Models

Due to the sheer number of survey items used in the literature without a theoretical rationale, I consider different plausible measurement models of political trust. I begin with a simple exploratory factor analysis (EFA) in each country using a broad range of commonly used indicators. From these solutions and some theoretical consideration, I construct four measurement models of political trust and subject them to tests of measurement validity and equivalence. While the EFA solutions are no substitute for theory, they strongly suggest that indicators as diverse as trust in the government, parliament, parties, police, armed forces and courts do *not* form a unidimensional model of political trust in most countries.[4]

Firstly, EFA output for approximately half of the countries suggests that a separate factor accounts for trust in regional and local political institutions, indicating that many citizens differentiate between local and federal levels of government. To test for this possibility, I construct Model 1 by specifying trust in the government, parliament, political parties, regional and local

[4] The EFA solutions are available in the online appendix.

government to load on a "political trust" latent variable, adding an error correlation between trust in the regional and local government. While the survey is unclear about what "regional" and "local" politics entail, it is likely that many respondents associate "regional" politics with the *rayon*, a Soviet-era administrative division of government slightly below the federal level which many (though not all) states retained after the collapse of the Soviet Union. The local level will likely correspond to city and town districts closer to the individual. Some variation might be expected between countries which inherited this structure from the Soviet Union and those which did not in Eastern or Central Europe. In Bulgaria, for instance, the *rayon* refers to a city-level rather than national government subdivision. If this model specification produces good fit to the data in most countries, we might conclude that local and regional political trust responses co-vary together in ways that cannot be accounted for by the political trust factor. At the same time, regional political trust might be more associated with federal-level political trust in former Soviet countries.

Secondly, EFA solutions show that citizens trust the police and armed forces in a different way than they trust political institutions like the government and parliament. Despite the lack of theory to support this dimension of political trust, the police and armed forces indicators form salient loadings on a separate factor in most countries in the sample. One possibility is that these institutions are the only ones which can legally exercise force to protect citizens. At least in principle, they may represent deeper notions of order that go beyond the tides of party politics, eliciting notions of patriotism or legitimacy which the parliament or government do not. To test for this "protective" dimensionality, I specify trust in the government, parliament, political parties, armed forces and police to load on a single factor, adding an error correlation between the latter two indicators. If this specification produces a good fit to the data in most countries, we can

conclude that respondents think about the police and armed forces differently than they think about political institutions.

Thirdly, as mentioned earlier, many authors argue that people evaluate the courts and police differently from the government or parliament because they are meant to be impartial and devoted to the maintenance of the rule of law and criminal justice. My EFA solutions do not consistently support this argument partly because trust in the courts loads on factors accounting for trust in political institutions like the government and parliament in authoritarian former Soviet states. It is likely that people living under politically repressive regimes might not believe that judicial and political institutions are independent of each other. I test whether this is the case by specifying trust in the government, parliament, political parties, courts and police to load on one 'political trust' factor with an error correlation between the courts and police. This specification may produce poorer fit in more politically repressive contexts.

Because indicators involving courts, armed forces, police and regional government are subject to error correlations and separate dimensions of political trust, they are likely to cause problems for cross-national equivalence. In the interest of capturing the largest amount of countries for an appropriate cross-national regression or mean comparison, in Model 4 I specify a simple political trust factor measured by trust in the government, parliament, political parties and local government. This model is closest to other CFA models of political trust using the European Social Survey and should produce the best fit to the data in most countries.

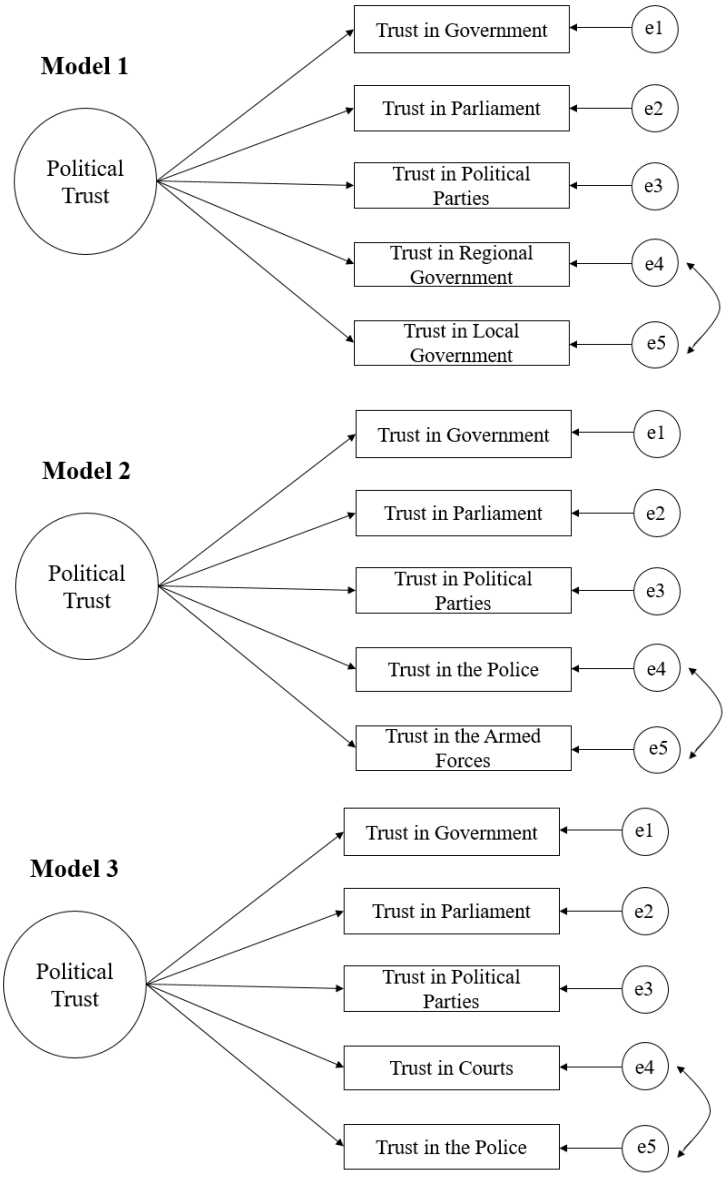
For ease of interpretation, I have included path diagrams of each measurement model in Figure 2.1. To check for the robustness of these models to alternative specifications, I compare each model to a bi-dimensional model in which the two items originally specified to have correlated errors are reflected by a separate factor. I also compare Model 4 to one excluding trust

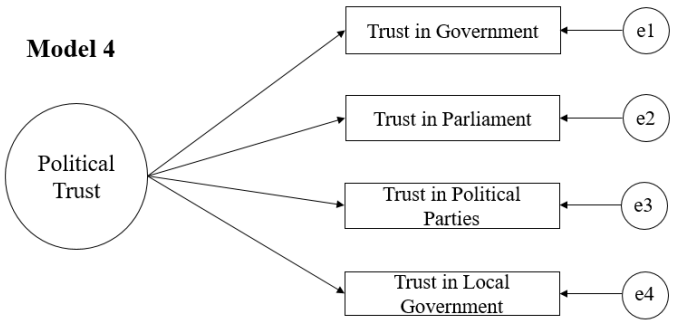
in the local government, keeping the factor strictly limited to federal-level political institutions. Across all models, I exclude the 'trust in the presidency/monarchy' indicator. British respondents would have been evaluating trust in the Queen, French respondents the French President and Belarusians an autocratic leader in power since 1994. A cross country comparison on such an indicator would be de-facto uninterpretable based on its heterogeneous content. This point finds confirmation in the EFA output, which does not show consistent factor loading patterns of the indicator across countries.

I begin with separate country CFAs and follow up with invariance testing on each model using Mplus 7. I run each model using Maximum Likelihood estimation, which takes into account all available data. When evaluating factor loadings, I consider a salient, standardized loading to be higher than 0.30 (Brown 2006). To assess each model's goodness of fit, I use several global fit statistics, including the chi square statistic, the RMSEA (root mean square error of approximation), the CFI (comparative fit index) and the SRMR (standardized root mean square residual). To reach acceptable fit, CFI should be greater than 0.95, SRMR below 0.10 and RMSEA below 0.08 (Schermelleh-Engel et al. 2003). I evaluate local model fit by considering expected parameter changes (EPC), modification indices (MIs) and the power of the MI test using the JRule for Mplus package (Oberski 2009).[5] This latter procedure is particularly important because traditional fit indices can prove very sensitive to sample sizes and other characteristics of models which make it easy to over- or under-estimate the sizes of model misspecifications. Following Saris et al.'s (2009, 562) recommendation, we can conclude, for instance, that there is no "substantively relevant" misspecification (i.e. a discrepancy between the model and fitted data) if the MI is significant but the power of the MI test is low.

[5] I set EPC values higher than 0.15 for unstandardized factor loadings and 0.20 for intercepts as cutoff criteria to assess metric and scalar invariance output. I specify the power of the test as 0.80 and a type I error of 0.05.

1 Figure 2.1 CFA Models





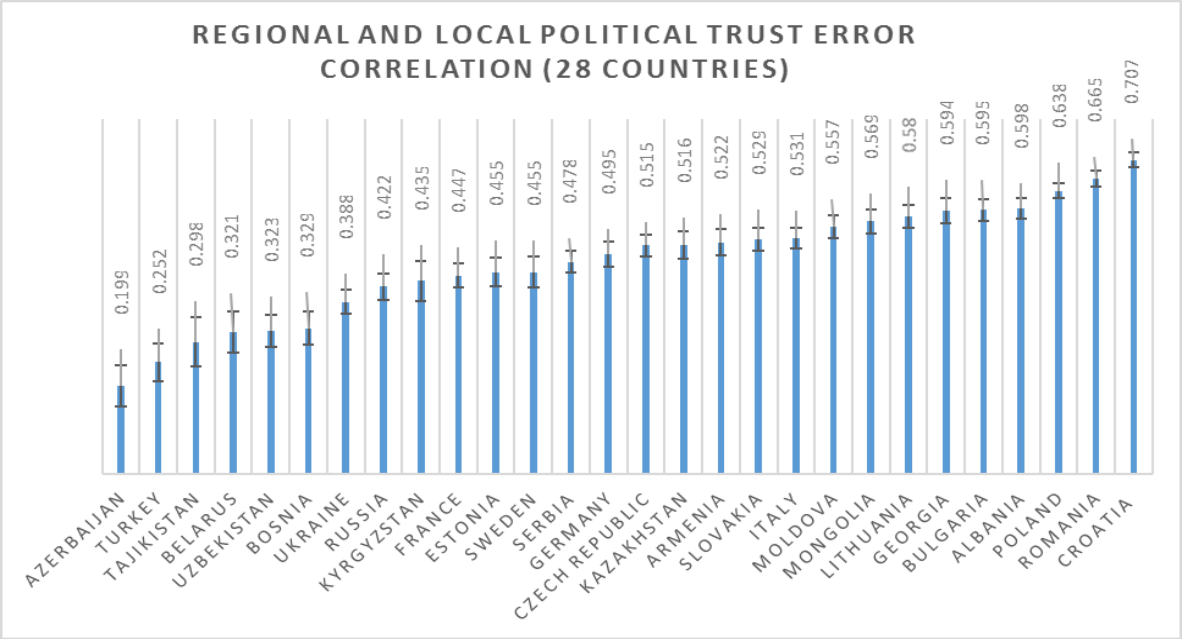
Since I use ML estimation on large sample sizes in each country, I do not use the chi-square difference test to assess the extent of the degradation of model fit between different levels of invariance. The full output, including standardized factor loadings and fit statistics for each country in each model, is available in the online appendix.

2.6 Results

2.6.1 Model 1

In Model 1, trust in the government, parliament, political parties, local and regional government load on one factor with an error correlation between the latter two indicators. Due to missing information on the regional trust indicator, Great Britain, Hungary, Kosovo, Latvia, Macedonia, Montenegro and Slovenia were not included in the analysis. Off the bat, there appears to be interesting variation in the way the model behaves across the surveyed territory. I ordered the countries by the ascending error correlation between regional and local political trust (Figure 2.2).

2 Figure 2.2 Regional and Local Political Trust Error Correlation



In the bulk of the former Soviet countries alongside Bosnia and Turkey, the extent to which regional and local political trust have a special relationship that cannot be accounted for by the political trust factor is relatively small, but gets progressively bigger in Western and Eastern Europe. Azerbaijan has the smallest error correlation (0.199), while Croatia has the largest (0.707). This means that local and regional political trust is decisively more related to trust in central political institutions in the former Soviet states than it is in Eastern and Western Europe. It is likely that regional and local political trust can load directly on a political trust factor without an error correlation in Azerbaijan, while in Croatia and select Eastern European countries, regional and local political trust might be modeled by a separate factor entirely. This could be due to different understandings of what constitutes "regional" politics or it could signify that citizens in former Soviet countries perceive that local and regional politics are subject to control by the federal center.

As expected, the same structure of the model is not present in several countries, preventing the model from reaching configural invariance across 28 countries. I use JRule to assist in detecting countries with the highest misspecifications. Several countries demonstrated more than one misspecification (for instance significant MIs recommend double error correlations between trust in the government and regional government, or political parties and parliament) which I chose not to correct for lack of a theoretical rationale. After removing Albania, Turkey, Kyrgyzstan, Russia, Tajikistan, Bulgaria and Estonia on these grounds, the model was able to achieve borderline configural invariance in 21 countries (chi sq=756.971, DF=84, RMSEA=.086, CFI=.990, SRMR=.014). These 21 countries are Armenia, Azerbaijan, Belarus, Bosnia, Croatia, Czech Republic, France, Georgia, Germany, Italy, Kazakhstan, Lithuania, Moldova, Mongolia, Poland, Romania, Serbia, Slovakia, Sweden, Ukraine and Uzbekistan. Though RMSEA is a bit higher than desired, JRule shows no misspecifications.

The model reached metric invariance with mostly acceptable fit statistics. After I freely estimated loadings for trust in political parties in Belarus, Uzbekistan and Georgia, trust in government in Azerbaijan and trust in local government and parliament in France, the model achieved partial metric invariance (chi sq=1244.946, DF=158, RMSEA=.079, CFI=.985, SRMR=.059). As expected, model fit deteriorated in the scalar invariance test. After I relaxed equality constraints on intercepts for trust in the government, local government and political parties in all countries and released one more factor loading constraint in four countries, the model achieved borderline acceptable partial scalar invariance (chi sq=1910.107, DF=174, RMSEA=.096, CFI=.975, SRMR=.074). Standardized factor loadings are substantial and significant in all countries. I have reported unstandardized factor loadings on the partial scalar

model in Table 5. It is possible to compare correlates of political trust across countries using this latent factor. Even mean comparisons are possible with some caution (Table 2.1).

I compared this model to a bi-dimensional model in which trust in the government, parliament and political parties is reflected by one factor and trust in the regional and local government is reflected by a second factor. It is reasonable to test the validity of this model particularly because several countries in the sample have a very high error correlation between the two dimensions. This model also reaches borderline partial scalar invariance with slightly better fit statistics across the same 21 countries (chi sq=1878.912, DF=184, RMSEA=.092, CFI= .976, SRMR=.057). Comparisons of correlates and cautious comparisons of means are also possible using this bi-dimensional measurement model of political trust.

1 **Table 2.1 Model 1: Error Correlation between Regional and Local Government (21 Countries)**

	Chi Square	**DF**	**RMSEA**	**CFI**	**SRMR**
Configural	756.971	84	0.086	0.990	0.014
Metric	1456.574	164	0.085	0.982	0.073
Partial Metric	1244.946	158	0.079	0.985	0.059
Scalar	3709.260	238	0.116	0.951	0.107
Partial Scalar	1910.107	174	0.096	0.975	0.074
Compare: Bi-dimensional model (regional and local trust reflect separate factor, 21 countries)					
Configural	756.968	84	0.086	0.990	0.014
Metric	1123.686	144	0.079	0.986	0.048
Scalar	2394.798	204	0.099	0.969	0.059
Partial Scalar	1878.912	184	0.092	0.976	0.057

Note. Data Source: EBRD and World Bank. All available data are used in ML estimation. DF= degrees of freedom, RMSEA= root mean square error of approximation; CFI= comparative fit index; SRMR=standardized root mean square residual.

2.6.2 Model 2

In Model 2, trust in the government, parliament, political parties, armed forces and police load on one factor with an error correlation between the latter two indicators. In this "protective" trust model, I test whether respondents distinguish between strictly political institutions and institutions which can use force to offer protection. Using JRule, I identified countries with multiple misspecifications (Belarus, Uzbekistan, Kyrgyzstan, Bulgaria, Kazakhstan and Estonia) and removed them before the model could reach configural invariance with 29 countries (chi sq= 692.945, DF=116; RMSEA=.067, CFI=.991, SRMR=.020). These 29 countries are Albania, Armenia, Azerbaijan, Bosnia, Croatia, Czech Republic, Macedonia, France, Georgia, Germany, Hungary, Italy, Kazakhstan, Kosovo, Kyrgyzstan, Latvia, Lithuania, Moldova, Mongolia, Montenegro, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Sweden, Tajikistan, Turkey, Great Britain and Ukraine.

When testing for metric invariance, I released factor loading constraints on trust in the police in Sweden, Azerbaijan and Armenia, armed forces in Sweden and Britain, government in Sweden and Kosovo, and political parties in Georgia and Kosovo. The model reached partial metric invariance with good fit statistics (chi sq=1242.068, DF=215, RMSEA=.066, CFI=.984, SRMR=.058). In the scalar invariant model, I released equality constraints on intercepts on trust in the police, government and armed forces in all countries, which resulted in acceptable partial scalar invariance (chi sq=1886.738, DF=243, RMSEA=.078, CFI=.974, SRMR=.062). As a result, it is possible to compare the correlates and means of political trust across 29 countries using this latent factor (Table 2.2). The unstandardized factor loadings on the partial scalar invariant model are available in Table 2.5.

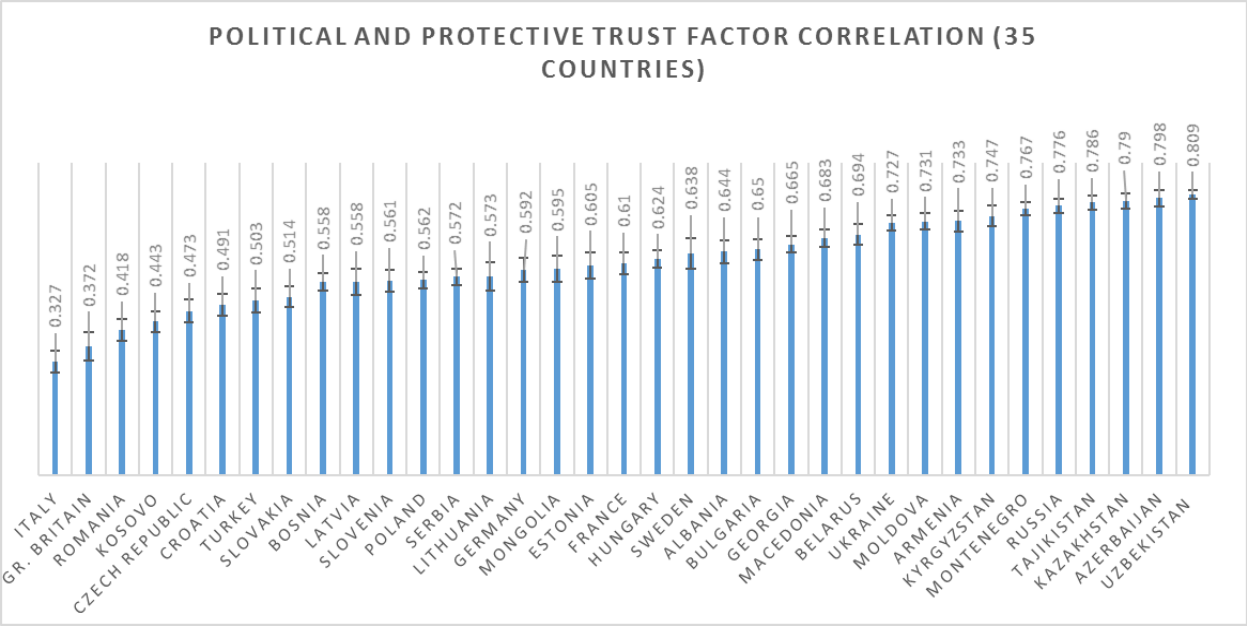
2 **Table 2.2 Model 2: Error Correlation between Armed Forces and Police (29 Countries)**

	Chi Square	DF	RMSEA	CFI	SRMR
Configural	692.945	116	0.067	0.991	0.020
Metric	1658.009	224	0.076	0.977	0.076
Partial Metric	1242.068	215	0.066	0.984	0.058
Scalar	7821.219	327	0.144	0.880	0.167
Partial Scalar	1886.738	243	0.078	0.974	0.062
Compare: Bi-dimensional model (armed forces and police reflect separate factor, 29 countries)					
Configural	692.945	116	0.067	0.991	0.020
Metric	1227.474	200	0.068	0.984	0.048
Scalar	4735.337	284	0.119	0.929	0.085
Partial Scalar	3967.392	256	0.115	0.941	0.079

Note. Data Source: EBRD and World Bank. All available data are used in ML estimation. DF= degrees of freedom, RMSEA= root mean square error of approximation; CFI= comparative fit index; SRMR=standardized root mean square residual.

A bi-dimensional model in which trust in the armed forces and police is reflected by a separate factor reaches metric invariance with better fit statistics than the unidimensional model, but it does not quite reach partial scalar invariance in the same countries (chi sq=3967.392, DF=256, RMSEA=.115, CFI=.941, SRMR=.079) because I was unable to release equality constraints on a factor with two indicators. As a result, one can use the bi-dimensional model to compare correlates, but not means across countries. If we line up the countries in order of the factor correlation in this bi-dimensional model, we can visualize some of the regional variation in this measurement model. In Figure 2.3, we can see that Italy has the weakest factor correlation (0.327) and Uzbekistan the largest (0.809).

3 **Figure 2.3 Political and Protective Trust Factor Correlation**



Again, there appears to be clustering by regime type. The weakest factor correlations appear in Eastern and Western Europe and the strongest in the former Soviet states. This means that trust in the police and armed forces has relatively little in common with trust in political institutions in the European part of the sample, and quite a lot in the former Soviet states. Importantly, factor correlations on the former Soviet side inching close to 0.80 show that there is a lack of discriminant validity between the factors. That is, trust in the police and armed forces are appropriate measures of political trust in these countries. Specifying an error correlation or two-factor model was likely part of the reason countries on this tail of the sample had poor fit statistics and had to be excluded from invariance testing in the first place.

2.6.3 Model 3

In Model 3, trust in the government, parliament, political parties, courts and police load on one factor with an error correlation between the latter two indicators. Here, I test whether respondents conceive "order" or "neutral" institutions to be separate from political institutions. As

in the previous models, the model would not reach configural invariance until I removed the countries with the most problematic misspecifications in JRule, where significant MIs recommend error correlations between trust in courts and political parties and between trust in parliament and government in Belarus, Bulgaria, Kazakhstan, Kosovo, Uzbekistan and Macedonia. The model still did not achieve configural invariance after removing these countries so I chose to remove countries with the next highest misspecifications in JRule suggesting complicated error correlations (Turkey, Bosnia, Azerbaijan, Moldova, Czech Republic and Ukraine). Unfortunately, the bulk of the excluded countries are former Soviet autocracies, making this model less comparable across regime types.

The amount of misspecification confirms the inconsistent loadings of trust in the courts in the initial EFA solutions: in most former Soviet states, parliaments and courts appear to be under the sway of central political institutions. This also appears in the standardized factor loadings (available in the online appendix). The five countries with the highest loadings for trust in courts are Azerbaijan (0.863), Armenia (0.851), Tajikistan (0.840), Montenegro (0.832) and Kosovo (0.780) while the ones with the lowest loadings are Sweden (0.442), Great Britain (0.446), Latvia (0.463), Italy (0.501) and Lithuania (0.528). Almost exactly the same pattern can be found regarding loadings for trust in the police. The five countries with the highest loadings are Azerbaijan (0.722), Montenegro (0.677), Uzbekistan (0.649), Tajikistan (0.646) and Kazakhstan (0.641) while the countries with the lowest loadings are Italy (0.290), Great Britain (0.309), Sweden (0.343), Lithuania (0.355) and Latvia (0.355). Respondents in former Soviet states and autocracies tend to associate courts and police with political institutions whereas respondents in Western Europe and the Baltics do not.

With 23 countries, the model reached acceptable fit statistics for configural invariance (chi sq=680.399, DF=92, RMSEA=.077, CFI=.987, SRMR=.020). The final 23 countries are Albania, Armenia, Croatia, Estonia, France, Georgia, Germany, Hungary, Italy, Kyrgyzstan, Latvia, Lithuania, Mongolia, Montenegro, Poland, Romania, Russia, Serbia, Slovakia, Slovenia, Sweden, Tajikistan and Great Britain.

The model did not reach metric invariance, so I used JRule to identify the most problematic items. After releasing factor loading constraints on trust in the government in Kyrgyzstan and Sweden as well as trust in the police and courts in all countries, the model reached partial metric invariance (chi sq=915.343, DF=134, RMSEA=.073, CFI=.983, SRMR=.042). The scalar invariance test produced very poor fit statistics. Although I released intercept equality constraints on trust in the police, government and political parties for all countries, the model failed to meet partial scalar invariance (chi sq= 2491.689, DF=156, RMSEA=.117, CFI=.950, SRMR=.112). The bi-dimensional model in which trust in the courts and police are reflected by a separate factor performed better on metric invariance than the unidimensional model but also failed to meet partial scalar invariance.

3 **Table 2.3 Model 3: Error Correlation between Courts and Police (23 Countries)**

	Chi Square	**DF**	**RMSEA**	**CFI**	**SRMR**
Configural	680.399	92	0.077	0.987	0.020
Metric	1580.448	180	0.084	0.970	0.082
Partial Metric	915.343	134	0.073	0.983	0.042
Scalar	4216.209	222	0.128	0.915	0.234
Partial Scalar	2491.689	156	0.117	0.950	0.112
Compare: Bi-dimensional model (courts and police reflect separate factor, 23 countries)					
Configural	680.397	92	0.077	0.987	0.020
Metric	1086.635	158	0.073	0.980	0.051
Scalar	3680.730	224	0.119	0.926	0.087
Partial Scalar	3028.274	202	0.113	0.940	0.078

Note. Data Source: EBRD and World Bank. All available data are used in ML estimation. DF= degrees of freedom, RMSEA= root mean square error of approximation; CFI= comparative fit index; SRMR=standardized root mean square residual.

On this basis we can use both models to compare the correlates of political trust across 23 countries, but not means (Table 2.3). The size of the error correlation across countries does not show clear geographical patterns, although, as expected, many former Soviet autocracies have to be excluded from invariance testing because the courts and police tend to load on federal-level political institutions. Interestingly, although this model has the most theoretical support in the literature for representing "order" or "neutral" institutions, it produces the poorest fit to the data within and across countries out of the four models mainly because the courts and police are *not* neutral or independent of political sway in a good part of the sample, contributing to disorder and undermining the rule of law rather than the reverse.

2.6.4 Model 4

In Model 4, trust indicators in the government, parliament, political parties, and local government load on a single 'political trust' factor. This specification achieved configural invariance for all 35 countries (chi sq=453.666, DF=70, RMSEA=.071, CFI=.994, SRMR=.013). Model fit deteriorated under the test for metric invariance, but after I released the problematic factor loading constraint on trust in the local government, the model achieved partial metric invariance (chi sq=905.601, DF=138, RMSEA=.072, CFI=.989, SRMR=.048). When testing for scalar invariance, I freed intercept equality constraints on trust in the local government and political parties in all countries, but the model barely failed to meet partial scalar invariance (chi sq=1777.389, DF=172, RMSEA=.093, CFI=.976, SRMR=.058). Though this can be considered a borderline case, JRule shows that significant misspecifications remain in nine countries. Any mean comparisons using this latent factor should be treated with caution.

4 **Table 2.4 Model 4: Simple (35 Countries)**

	Chi Square	DF	RMSEA	CFI	SRMR
Configural	453.666	70	0.071	0.994	0.013
Metric	1522.867	172	0.085	0.980	0.076
Partial Metric	905.601	138	0.072	0.989	0.048
Scalar	3973.355	240	0.120	0.945	0.090
Partial Scalar	1777.389	172	0.093	0.976	0.058
Compare: Simple model without trust in local government, 35 countries					
Configural	0.001	0	0.000	1.000	0.000
Metric	432.608	68	0.070	0.992	0.052
Scalar	2274.023	136	0.120	0.953	0.075
Partial Scalar	1293.938	102	0.104	0.974	0.058

Note. Data Source: EBRD and World Bank. All available data are used in ML estimation. DF= degrees of freedom, RMSEA= root mean square error of approximation; CFI= comparative fit index; SRMR=standardized root mean square residual.

A comparative model with only three indicators (excluding trust in local government) also failed to meet partial scalar invariance (chi sq=1293.938, DF=102, RMSEA=.104, CFI=.974, SRMR=.058). As expected, a simple model of political trust without multidimensional indicators was able to reach partial metric invariance across all countries, proving the most conducive of all models to a cross-national pooled regression analysis using this survey. Comparing means of political trust using either of these simple models, however, may be problematic due to the lack of partial scalar invariance. Invariant unstandardized factor loadings for this model are available in Table 2.5.

5 **Table 2.5 Unstandardized Factor Loadings on Partial Metric and Scalar Invariant Models**

Item	**Model 1** Partial Scalar 21 countries	**Model 2** Partial Scalar 29 countries	**Model 3** Partial Metric 23 countries	**Model 4** Partial Metric 35 countries
Trust in Government	1.000 (0.000)	1.000 (0.000)	1.000 (0.000)	1.000 (0.000)
Trust in Parliament	0.981 (0.006)	1.025 (0.007)	1.063 (0.008)	1.024 (0.006)
Trust in Political Parties	0.774 (0.007)	0.780 (0.006)	0.803 (0.008)	0.781 (0.006)
Trust in Local Gov.	0.848 (0.007)			0.745 (0.036)
Trust in Regional Gov.	0.898 (0.006)			
Trust in Courts			0.831 (0.034)	

Trust in Police	0.566 (0.007)	0.514 (0.040)
Trust in Armed Forces	0.525 (0.007)	

Note: All available data are used in ML estimation. Loadings are all significant (p<0.01)

Because the four-indicator simple model managed to reach partial metric invariance for all 35 countries in the sample, I have also included unstandardized factor loadings per country for this model in Table 2.6. A perusal of these results helps illustrate why trust in the local government proved to be the most problematic indicator during invariance testing. The five countries with the highest loadings for trust in local government are Tajikistan (1.231), Kyrgyzstan (1.164), Azerbaijan (1.127), Uzbekistan (1.054) and Russia (1.031), while the countries with the lowest are France (.511), Estonia (.512), the Czech Republic (.605), Latvia (.628), Slovakia (.650) and Lithuania (.672). Unsurprisingly, we see that respondents from countries in Central Europe and the Baltics distinguish between local and federal levels of government, while those in former Soviet autocracies do not. This is consistent with the results in Models 1 and 3.

6 **Table 2.6 Unstandardized Factor Loadings Per Country, Model 4**

Country	Trust in Government	Trust in Parliament	Trust in Political Parties	Trust in Local Government
Albania	1.000 (0.000)	1.091 (0.046)	0.819 (0.041)	0.770 (0.041)
Armenia	1.000 (0.000)	1.002 (0.027)	0.791 (0.031)	0.887 (0.032)
Azerbaijan	1.000 (0.000)	1.168 (0.038)	0.948 (0.038)	1.127 (0.045)
Belarus	1.000 (0.000)	0.957 (0.026)	0.606 (0.036)	0.954 (0.028)
Bosnia	1.000 (0.000)	1.035 (0.022)	0.784 (0.026)	0.994 (0.025)
Bulgaria	1.000 (0.000)	0.978 (0.040)	0.802 (0.037)	0.824 (0.046)
Croatia	1.000 (0.000)	1.030 (0.034)	0.809 (0.033)	0.849 (0.042)
Czech Rep	1.000 (0.000)	1.072 (0.041)	0.817 (0.036)	0.605 (0.042)
Estonia	1.000 (0.000)	1.231 (0.084)	0.643 (0.052)	0.512 (0.050)
France	1.000 (0.000)	1.009 (0.059)	0.660 (0.042)	0.511 (0.047)
Georgia	1.000 (0.000)	1.005 (0.024)	0.567 (0.032)	0.973 (0.026)
Germany	1.000 (0.000)	1.002 (0.033)	0.703 (0.032)	0.791 (0.031)
Gr. Britain	1.000 (0.000)	1.021 (0.030)	0.711 (0.026)	0.783 (0.030)
Hungary	1.000 (0.000)	0.963 (0.031)	0.673 (0.028)	0.774 (0.032)
Italy	1.000 (0.000)	1.005 (0.033)	0.826 (0.032)	0.736 (0.039)

Kazakhstan	1.000 (0.000)	0.951 (0.026)	0.759 (0.032)	0.863 (0.027)
Kosovo	1.000 (0.000)	1.172 (0.039)	1.054 (0.041)	0.976 (0.037)
Kyrgyzstan	1.000 (0.000)	1.131 (0.078)	0.966 (0.073)	1.164 (0.072)
Latvia	1.000 (0.000)	1.043 (0.055)	0.718 (0.044)	0.628 (0.053)
Lithuania	1.000 (0.000)	0.993 (0.052)	0.755 (0.045)	0.672 (0.050)
Macedonia	1.000 (0.000)	0.993 (0.037)	0.750 (0.035)	0.937 (0.038)
Moldova	1.000 (0.000)	0.969 (0.022)	0.815 (0.026)	0.774 (0.028)
Mongolia	1.000 (0.000)	1.035 (0.050)	0.868 (0.047)	0.833 (0.047)
Montenegro	1.000 (0.000)	0.862 (0.022)	0.655 (0.027)	0.854 (0.024)
Poland	1.000 (0.000)	1.056 (0.030)	0.814 (0.029)	0.831 (0.028)
Romania	1.000 (0.000)	1.142 (0.046)	0.757 (0.040)	0.912 (0.055)
Russia	1.000 (0.000)	1.126 (0.036)	0.819 (0.033)	1.031 (0.036)
Serbia	1.000 (0.000)	1.062 (0.028)	0.804 (0.026)	0.892 (0.031)
Slovakia	1.000 (0.000)	1.021 (0.034)	0.838 (0.034)	0.650 (0.036)
Sweden	1.000 (0.000)	0.901 (0.049)	0.698 (0.043)	0.780 (0.045)
Tajikistan	1.000 (0.000)	1.243 (0.044)	1.067 (0.055)	1.231 (0.045)
Turkey	1.000 (0.000)	0.963 (0.039)	0.640 (0.040)	0.866 (0.036)
Ukraine	1.000 (0.000)	0.969 (0.027)	0.737 (0.026)	0.764 (0.030)
Uzbekistan	1.000 (0.000)	1.050 (0.019)	0.998 (0.032)	1.054 (0.021)

Note: Note: All loadings are significant ($p<0.01$)

What would have happened if we were to rank countries by their raw means or sum scores instead? The simple model (model 4) did not quite reach partial scalar invariance, so a comparison of latent means using this model *should be conducted with caution*. Nevertheless, in a simple demonstration, we can observe that country rankings using latent means from a model close to partial scalar invariance are slightly different than rankings based on a simple mean of trust in the government, parliament, political parties and local government (Table 2.7).

The top and bottom of the list remain relatively similar (the five least politically trusting countries being Romania, Croatia, Lithuania, Latvia and Serbia, and the five most politically trust being Belarus, Kazakhstan, Sweden, Tajikistan and Uzbekistan), although several countries shift one or two spots. When using latent means, Macedonia and Armenia move up in their average trust ranking by three spots and Russia by five spots. In other words, after "controlling" for measurement error, these countries are actually more trusting of political institutions than we

would have concluded by simply averaging the indicators. Beside this, the general consistency between the rankings is not too surprising because model 4 contains the least problematic indicators. *With the indicators that we have*, there is no alarming reason to preclude countries as different as Sweden and Uzbekistan from a comparative study of political trust. We have seen, however, that more complex measurement models are prone to more measurement error which can induce bias in mean comparisons and pooled regressions. The sources of error, if systematic, should not be assumed unproblematic prior to a study.

Table 2.7 Raw versus Latent Means (Model 4)

Country	Raw Mean	Country	Latent Mean
Romania	20.343	Romania	-0.944***
Croatia	23.680	Croatia	-0.677***
Serbia	27.197	Lithuania	-0.534***
Lithuania	29.017	Latvia	-0.512***
Latvia	29.403	Serbia	-0.463***
Bosnia	30.664	Bosnia	-0.346***
Bulgaria	32.184	Italy	-0.296***
Italy	32.345	Ukraine	-0.296***
Macedonia	33.306	Bulgaria	-0.295***
Ukraine	33.576	Slovenia	-0.292***
Moldova	34.389	Moldova	-0.241***
Slovenia	34.890	Macedonia	-0.226***
Great Britain	35.515	Czech Republic	-0.207***
Armenia	37.202	Great Britain	-0.179***
Czech Republic	37.294	Slovakia	-0.120***
Slovakia	37.880	Kosovo	-0.083*
Kosovo	38.867	Armenia	-0.060
Albania	39.289	Kyrgyzstan	-0.049
Kyrgyzstan	39.531	France	-0.025
France	40.413	Albania	0.000
Russia	42.072	Poland	0.189***
Mongolia	43.031	Mongolia	0.202***
Poland	44.425	Germany	0.204***
Germany	44.946	Estonia	0.245***
Hungary	45.536	Hungary	0.270***
Estonia	47.503	**Russia**	0.345***
Georgia	50.620	Montenegro	0.519***
Montenegro	50.650	Georgia	0.585***
Azerbaijan	53.261	Azerbaijan	0.725***

Belarus	54.006	Turkey	0.781***
Turkey	57.5	Belarus	0.787***
Kazakhstan	58.242	Kazakhstan	0.917***
Sweden	60.713	Sweden	0.981***
Tajikistan	73.393	Tajikistan	1.569***
Uzbekistan	82.193	Uzbekistan	1.801***

2.7 Discussion

Despite growth in comparative political trust research in the last two decades, scholars have paid insufficient attention to the measurement validity and cross-national equivalence of the concept. The most common approach to measurement has consisted of taking averages or sum scores of diverse sets of indicators without theoretical justification. In this paper I showed that this common "kitchen sink" approach to measurement is inappropriate by investigating the measurement equivalence of political trust across thirty-five countries in Europe and the former Soviet space using the 2010 Life in Transition II Survey. Although issues in cross-national measurement are gaining attention in political trust research, this is the first study to examine the measurement validity and equivalence of political trust across diverse regime types.

I tested four models of political trust, finding that trust perceptions in political institutions like the government, parliament and political parties tend to differ from 1) trust in regional and local political institutions, 2) trust in protective institutions like the armed forces and police and 3) trust in order institutions like the courts and police. Measurement models with error correlations along these dimensions of political trust all reached at least partial metric invariance across *most* countries in the survey. This means that respondents in diverse cultures and regime types understand subsets of survey questions similarly, which allows us to compare the correlates of the latent factors from each model without losing substantive meaning. Coefficient estimates in a pooled regression analysis will not suffer from measurement-induced bias if these models of

political trust are specified correctly and used within the structural equations framework to control for measurement error. It will not be enough to use sum scores or averages of each model's combination of indicators.

While this outcome allows us a fair degree of optimism about the comparability of the measurement models, the only model which was comparable across all thirty-five countries was based on just four indicators (trust in the government, parliament, political parties and local government). A handful of countries, usually former Soviet autocracies, had to be excluded from invariance testing in the other models. Some variation in error correlations proved to be non-trivial across regime types. I found that trust in local and federal-level political institutions has relatively little in common in Eastern Europe, mildly more in Western Europe, and significantly more in the former Soviet space. Political trust also tends to be unrelated to trust in the police and armed forces across Europe, but strongly related in the former Soviet space, suggesting the effects of corruption and stronger central controls over these institutions. Likewise, citizens of former Soviet countries do not always perceive courts to be independent of political influence.

Two models achieved partial scalar invariance, allowing for mean comparisons on the latent factor across subsets of countries. The thirty-five country simple model of political trust, however, barely failed to reach partial scalar invariance according to conventional fit statistics. While this may appear disappointing for the prospects of cross-regime mean comparison, some advances in latent variable modeling provide reason for optimism. Oberski (2014) shows that a lack of invariance might not necessarily invalidate group comparisons and introduces the EPC-interest statistic to assess the substantive relevance of invariance misspecifications. A number of studies discuss the possibility of Bayesian techniques to establish approximate measurement invariance where traditional fit statistics appear to be overly strict (Muthén and Asparouhov 2012;

Davidov et al. 2015; van de Schoot et al. 2013; Zercher et al. 2015). Using these tools in cross-regime surveys to improve invariance testing can be a fruitful direction for future research.

2.7.1 Responding to Inglehart and Welzel's Critique

Before concluding this section I wish to address a recent critique of MGCFA which purports to overturn the entire paradigm of measurement equivalence testing. After Alemán and Woods (2015) used MGCFA to demonstrate the lack of cross-national equivalence in Inglehart and Welzel's measurements of emancipative and secular value orientations in the World Values Survey, Inglehart and Welzel (2016) made a biting response, urging readers to consider a fundamental paradigm shift in the way we study measurement equivalence. Their critique, in my view, misses the point of the existing paradigm in several ways.

At the heart of their argument, Inglehart and Welzel claim that "a construct's measurement features at the individual level provide no information whatsoever about the same construct's validity at the country level," demonstrating that inter-item correlations can be low among individuals but high between countries due to "background noise…that partially obscures true correlations." In other words, within-country variability in attitudinal measures in no way invalidates the aggregation and cross-country comparisons of emancipative values. Since individual level variation in the World Values Survey measures is randomly distributed, they argue, aggregation cancels out this noise. Indeed, there is a substantive rationale for ignoring individual-level convergence patterns: the purpose of measuring emancipative values is "not to measure internally convergent personality traits" but to capture a "culture-type phenomenon that only surfaces in the aggregate and, hence, does not exist at the individual level" (ibid., 4). The authors argue that "comparability properly understood boils down to external linkages, not internal

convergence. And external linkages is entirely a matter of a construct's association with its expected correlates" (ibid., 10). So if we find that emancipative values correlate with other aspects of reality like democracy, we need not worry about the measurement equivalence of the construct across countries. "Only" if the lack of internal consistency in the measures "obscures the external linkages… could one infer incomparability from variability in coherence" (ibid.).

There are several problems with this argument. First, while the authors advocate for a new paradigm in measurement equivalence, they do not discuss the existing one. Contrary to their diagnosis, measurement equivalence is not about the variability of attitudes at the individual level, and even less so about personality traits. In the way that we currently use the term "equivalence," we wish to establish that two people in different countries holding the same position on the construct will score the same thing on the survey. Different scores from people holding the same position can arise from different understandings of the construct, faulty translations or other features of the way the survey was conducted. When such nuisance effects infiltrate the observed scores, we are no longer comparing true attitudes across societies. And when it comes to using them to explain real world phenomena, biased measures can help over- or under-estimate regression coefficients. Nowhere in the article do the authors mention this notion of measurement equivalence or address the possibility of such measurement-induced bias. Instead, the authors presume that cross-national surveys deliver attitudinal measures which mean precisely what they want them to mean. Observed measures can immediately be generalized to the level of the latent construct ("emancipative values") which they are meant to reflect.

The authors justify this approach by arguing that "collective mentalities" which "represent a culture-type phenomenon" arise only at the aggregate level. While this is true, the authors do not recognize that such cultural attitudes can affect the measurement process itself. As Ariely and

Davidov (2011, 272) note in their study of the measurement equivalence of attitudes toward democracy, "the very differences in culture that give such cross-national studies their value also threaten the achievement of equivalence of scales that are used to measure the concept…" Precisely because culture can influence a survey respondent's understanding of complex phenomena like democracy, secularism or emancipation, we must be wary of the survey instrument's metric and scalar invariance properties if we wish to be sure that emancipative values refer to the same idea for individuals in two different countries. Because they are inherent to entire societies and subgroups, culturally-framed interpretations of survey questions can produce systematic, not random measurement errors, and such errors cannot be canceled out through simple aggregation.

The authors' argument that measurement equivalence boils down to the construct's association with its expected correlates, or "external linkages," is hardly a solution. In this view, any hodgepodge of summed indicators is cross-nationally valid if it happens to correlate with other variables. One need not worry about what the hodgepodge actually means. Spurious correlations can be found on the basis of spurious measurement. This is not difficult to accomplish. As long as there is a non-zero partial correlation between constructs given a large sample size (which the WVS provides), the power to detect a significant correlation between them tends to be large. The authors' demonstration that emancipative values significantly predict "effective democracy" scores across 100 countries while controlling for the internal consistency of emancipative value indices using Cronbach's alphas (ibid., 12) only shows us that significant correlations can easily be detected, not that the survey delivers comparable scales of emancipative values. Like an exploratory factor analysis or principle components analysis, Cronbach's alpha measures the average correlation of a set of items. The procedure can produce high internal consistency scores

even if the items are spuriously related. These popular exploratory procedures may be used in the preliminary stages of measurement model building, but they are in no way fundamental or even necessary for establishing the measurement validity or equivalence of a construct. CFA, unlike these procedures, requires that items correspond to a theoretically-sound measurement model, and MGCFA tests whether the data correspond equivalently to that model across populations.

Even supposing that internal consistency in the exploratory sense was a fundamental criterion for measurement validity, the authors claim that the "only" case in which the lack of internal consistency in the items will result in cross-national non-equivalence is if the set of measures does not in fact produce the "external linkages" we would theoretically expect (ibid., 10). In other words, only if we do not observe correlations of emancipative values with variables like effective democracy or protest activity can we question the measurement validity of emancipative values on the basis of their internal consistency. This seems highly speculative. One can presume that the authors recognize the possibility that measures are not equivalent across populations, but are willing to look into the matter only when the measures don't produce the desired theoretical outcomes. Methodological considerations can be overlooked as long as measures taken at face value produce the desired correlations. Even supposing that the correlations or "external linkages" they wish to establish are based on perfectly measured variables, upon failing to discover a significant correlation the authors would first conclude that there is a problem with the measurement of the constructs rather than the theory itself. At what point then, will the presence of disconfirming evidence (i.e. a lack of expected external linkages) lead the authors to part with the theory?

Toward the end of the article the authors finally mention the possibility of construct bias, or the idea that people may have different understandings of emancipation across countries. They

dismiss this concern in a few sentences: “The WVS directly addresses such down-to-earth topics as male dominance, child obedience, and heterosexual norms. It is hard to believe that people have no first-hand experience with such fundamental realities of everyday life” (ibid., 17). Simply because this possibility is “hard to believe” and because the measures are “down to earth,” there is apparently no need to check that different understandings of the concept (much less the data collection process) could have had an influence on the way individuals scored on the survey questions across more than 100 countries.

This misunderstanding becomes most revealing when the authors discuss how methodologists in the current paradigm would treat the hypothetical relationship between life satisfaction and perceived freedom: “In the eyes of MGCFA, the key question here would be whether life satisfaction and perceived freedom represent a single latent variable, something like a higher-ordered subjective wellbeing factor. To answer this question, MGCFA examines whether the two variables relate to each other in the same way within each group. Thus, the group mean becomes the standard of reference while differences between groups are ignored. Doing so assumes that the location of group means does not matter when in fact this might make a big difference…” (ibid., 17).

To dissect this point, it is first worth mentioning that MGCFA is only a statistical tool; it has no “eyes” with which to determine how to solve such a question. There is no obvious way to approach a question about the relationship between life satisfaction and perceived freedom from *any* methodological point of view without first examining the theoretical nature and significance of the puzzle, which the authors do not present. It would appear that from their perspective, MGCFA practitioners treat research questions entirely a-theoretically, as a doctor might propose a set of surgical tools without examining the site of the patient’s wound. The reality is quite the

opposite: MGCFA forces us to check whether our measures indeed correspond empirically in different contexts to what we believe they should correspond.

Since the authors are unclear about whether the relationship between life satisfaction and perceived freedom is an issue of measurement or causality, there is no reason to believe in the hypothetical outcomes they outline. Regardless, the authors complete the thought experiment by claiming that "MGCFA would draw the conclusion that life satisfaction and perceived freedom do not reflect a common well-being dimension because they do not co-vary everywhere in the same way relative to the given group mean" (ibid., 18-19). Not only do they fail to provide justification for using the method in the first place, but they claim to know what the outcome would be without actually running the analysis!

Again, the thought experiment is based on another flawed notion of how the technique works: "the group mean becomes the standard of reference while differences between groups are ignored." In fact, from the vantage point of the current measurement equivalence paradigm, we cannot entertain group mean comparisons until we have established that the chosen measures for the construct are at least partially scalar invariant. Contrary to what Inglehart and Welzel argue, MGCFA does not eliminate substantive variation in a construct across groups. It tests whether the survey instrument delivers comparable measures so that any mean comparisons or regression analyses using the construct will be based on true scores rather than biased scores.

The authors could have a valid point by arguing that measures of emancipative values are formative, not reflective indicators. That is, rather than reflecting a higher order dimension, the measures form an additive index that need not be internally correlated. If this is truly the case, Alemán and Woods' (2015) argument and the use of MGCFA would not hold water. But Inglehart and Welzel only mention in a passing sentence that "Welzel's (2013, 60) measure of emancipative

values is introduced explicitly as a combinatory construct, not a dimensional one" without explaining why (ibid., 8). At the very least, they could have re-introduced the original rationale and argued why dropping any indicator would substantively change the meaning of the emancipative values construct. It is hard to accept this argument without further justification, although if properly defended, this point alone would be enough to challenge Alemán and Woods' study without having to undertake a flawed critique of MGCFA.

While the authors believe they are challenging MGCFA or what they call "the new booming industry in cross-national survey research" (ibid., 6), they are only misconstruing the concept of measurement equivalence and its statistical functions. This neither challenges the existing measurement paradigm nor proposes a new one. In saying this I do not mean to reject Inglehart and Welzel's research program on postmaterialist values, which has proven highly influential in comparative politics. The way in which emancipative-traditional values map onto world regions in a theoretically predictable way is an important finding, and can only be bolstered if approached with some thoughtfulness about the cross-national measurement properties of the survey instrument. It is not in the interest of science to insist that measures be taken at face value as long as they produce desired correlations. The pathway of inference from data collection to the causal model, interpretation of results and policy recommendations should be questioned at each step, although the quality of the entire process depends overwhelmingly on step one. We should not treat survey instruments or data collectors as carriers of perfectly reliable information about the social world. In much the same way that we can challenge the ability of randomized control trials to deliver causal interpretations of treatment effects, we should question the ability of cross-national surveys to deliver meaningfully comparable measures in heterogeneous contexts. MGCFA is just one way of dealing productively with this complication.

2.7.2 Conclusion

In the context of the current study, many interesting questions remain about what political trust truly means. While factor analysis illuminates the regional clustering of measurement patterns using typical survey questions, it cannot determine the nature and content of the studied beliefs, nor the precise reasons for misfit. Indeed, the test of the cross-national validity of political trust in this investigation is limited to a test that the various institutional indicators actually measure what one would call "political" trust. We know from this study that not all institutions are treated as equally "political" by respondents across the regime spectrum, but we can still infer little about the higher-level construct validity of the measures if we are interested in the foundations of institutional trustworthiness or legitimacy. That is, we do not know whether the measures correspond to notions of the normative appropriateness of institutional performance, or whether those notions are similar across societies.

Probing studies or anchoring vignettes (King et al. 2004) can be useful to capture such local knowledge about political trustworthiness and detect qualitative reasons for measurement inequivalence across societies. If one only has access to traditional Likert-scale political trust questions, however, it is important to improve the way we use them in cross-national research. Factor analysis, among other latent variable modeling methods, can help in this regard. Although political trust is believed to have profound consequences for how we are governed and relate to each other, we cannot properly assess its causes and effects without diligently accounting for its measurement validity in diverse institutional contexts. Having established the measurement equivalence of political trust across the former Soviet space, I will now proceed with a substantive analysis of the relationship between political trust and corruption.

3 Chapter 3. A Theory of Corruption Tradeoff

3.1 Introduction

Does corruption make us less likely to trust political institutions? Comparative investigations of this question have shifted from a resounding "yes" to more uncertain prognoses in the last decade. According to conceptions of political legitimacy favorable to Bo Rothstein's (2009) interpretation, corruption and political trust are two sides of the same coin. If legitimacy is determined by what he calls "procedural fairness based on impartiality," deviation from these norms is by definition a form of corruption. Such corruption or partiality in public administration can be manifested in a number of ways, including the forceful extraction of bribes, discrimination or the use of unofficial networks to channel public resources to favored parties. These actions are said to break an implicit contract which makes public servants accountable to the electorate. Problematically, this widely accepted concept and theory of trust does not always bear out in the data. As the study of trust and corruption has expanded to non-European and developing country samples, there is increasing evidence that people express support for political institutions even in the presence of corruption (Fernández-Vázquez et al. 2014; Manzetti and Wilson 2006; Zechmeister and Zizumbo-Colunga 2013). This emerging finding raises interesting questions about how governments retain public support in dysfunctional institutional settings, challenging long-reigning explanations of the causes and consequences of political trust.

In this paper I test the external validity of theories explaining high-trust, high-corruption trends in a sample of 30 countries which span the former Soviet Union, Eastern Europe and Central Europe. Though poor data availability has largely prevented the comprehensive study of public opinion across the entire former Soviet space in the decades following the Soviet collapse, recent survey data suggest that citizens in rarely studied parts of Central Asia and the southern Caucasus

have extraordinarily high trust in political institutions despite high petty corruption perceptions. In select countries in the region, this puzzling trend has coincided with significant economic growth stemming from natural resource rents. I hypothesize that people who perceive high corruption in such circumstances trust political institutions for similar reasons as their counterparts in Latin American clientelist democracies: they trade off concerns about corruption when they are materially prospering and their country is growing. Such an incentive structure, if truly operative, can lead to suboptimal conditions for institutional reforms in corrupt systems of governance across the regime spectrum.

I also recognize that the post-communist space from Central Europe to Central Asia is highly institutionally diverse and posit that democratic mechanisms can moderate the way people process and react to corruption. My second main hypothesis is that citizens of democracies will hold political institutions more accountable for political outcomes than citizens of autocracies, resulting in greater penalties on political institutions for petty corruption. While many studies of trust and corruption remain limited to culturally homogenous world regions, using this diverse regional space can help us further theorize about the ability of governments under different regime settings to shield themselves from public corruption grievances.

To test these theories, I investigate interactions of petty corruption perceptions with economic context and regime settings. To account for measurement equivalence and the heterogeneity of corruption effects across countries and subnational regions, I test fixed effects OLS models for robustness against alternative specifications including multilevel random coefficient and structural equation models. To my surprise, I discover that neither of my hypotheses are supported by the data. First, people do not trade off concerns about corruption under any conditions of economic flourishing or personal financial satisfaction. Second, citizens

of autocracies who perceive high levels of corruption are significantly *more* distrusting of political institutions than those who perceive similar levels of petty corruption in democracies. While a theory of corruption tradeoff suggests that governments can supply short term economic gains to appease the public, this does not appear to be the case in the former Soviet space. This finding recommends an alternative perspective on the stability of authoritarian regimes in the region.

3.2 Trading Off Corruption Concerns for Material Gains

Corruption tends to reduce a person's trust in political institutions across Eastern Europe and the Baltic states (Mishler Rose 1997, 2001; Rose et al. 2005; Luhiste 2006), developed democracies (Anderson and Tverdova 2003), Latin America (Seligson 2002), East Asia (Chang and Chu 2006; Kim and Voorhees 2011), Sub-Saharan Africa (Cho and Kirwin 2007; Lavallée et al. 2008) and in a general global sample of 103 countries (Clausen et al. 2012). While these studies are distinct in the way corruption is measured, they tend to suggest fairly consistently that paying bribes or witnessing the abuse of public office for private gain makes one less likely to trust political institutions. If political legitimacy depends on what Rothstein (2009, 323-5) calls "procedural fairness" or impartiality in policy implementation, any state which "systematically departs" from this ideal in the form of "corruption, discrimination and similar violations...will be seen as illegitimate." A politician's diversion of public funds to favored parties or personal use is seen to fundamentally violate her mandate to allocate such funds in the interests of the electorate.

For a growing part of the comparative literature on corruption, this theory of trust depends on an unduly narrow interpretation of what constitutes good institutional performance to the electorate. While Rothstein's conception of political legitimacy as procedural fairness resonates in advanced democracies, it does not necessarily apply in other contexts where corruption is seen as complementary or even essential to citizens' expectations of political institutions. Across the

developing world, corruption can be regarded as a net benefit or at worst a tolerable nuisance when citizens engage in a form of economic voting. Indeed, positive evaluations of the economy are the most common predictor of high political trust across a wide range of cultures even after controlling for relevant sociopolitical variables (Anderson and Tverdova 2003; Askvik and Ishtiaq 2013; Catterberg and Moreno 2005; Chang and Chu 2006; Cho and Kirwin 2007; Hetherington 1998; Lavallée et al. 2008; Lewis-Beck and Stegmaier 2008; Luhiste 2006; Mishler and Rose 1997, 2001; Yang and Tang 2010).

How does economic context influence the way citizens punish political institutions for corruption? In among the earliest investigations of economic voting, Rundquist, Strom and Peters (1977, 961) argue that clientelism, or a public official's selective exchange of material benefits for loyalty, insufficiently explains why people vote for corrupt politicians. Instead, they argue that people are willing to trade off concerns about corruption if a corrupt politician holds a position about which people feel particularly strongly, which is often tied to expectations of economic improvement. They find that voters punish elected officials for corruption more often during economic downturns. As comparative survey data flourished almost four decades later, this seminal study encouraged scholarship on the notion of corruption tradeoff most visibly in Latin America.

Manzetti and Wilson’s (2006) analysis of Argentina, for instance, shows that corruption perceptions have a negative impact on political trust only when people are unhappy with economic conditions. Zechmeister and Zizumbo-Colunga (2013) confirm this in their study of economic context as a moderator of the relationship between corruption perceptions and presidential approval in 19 Latin American countries. In regions experiencing steep losses of income, people are more likely to link grand corruption perceptions to the president's performance than in more

prosperous regions. Carlin et al. (2015) use time series presidential approval data across 84 administrations in 18 Latin American countries to show that people tend to downgrade their approval during corruption scandals only in times of high unemployment and inflation. Rosas and Manzetti (2015) also estimate the moderating effects of country level macroeconomic conditions on the tendency for Latin American citizens to link personal bribe victimization to presidential performance. While the authors show that victims of bribery disapprove of presidents even in conditions of economic growth, they find some evidence that bribe victims are less punitive of presidents in conditions of monetary stability and high employment.

Muñoz et al. (2016) find similar effects from survey experiments in Catalunya, showing that people tolerate grand corruption when politicians present a record of delivering prosperity *and* when the parties in question refuse to accept charges of corruption. Fernández-Vázquez et al. (2014) examine the effects of different kinds of corruption scandals on electoral outcomes across Spain, finding more evidence for a traditional clientelist model of corruption voting in which material benefits mediate the trust-corruption nexus. Unlike most studies relying on observational data, the authors avoid perceptions variables and opt to measure corruption in terms of actual instances of embezzlement and its welfare enhancing or welfare reducing effects on electoral support. They find that while the vote share is reduced for all corrupt (as opposed to honest) mayors, the loss of votes is less steep for corrupt mayors who share rents with citizens. It would appear that the Brazilian phrase “Rouba maz Faz” (the politician “steals, but gets things done”) appropriately describes the rationale for corruption voting in a number of contexts around the globe. Interestingly, the phrase appears to be less resonant in its country of origin, Brazil, based on survey experiments conducted by Winters and Weitz-Shapiro (2012), which indicate that citizens are unwilling to condone corruption even if told that the mayor has delivered substantial

material benefits to residents of their hometown. Konstantinidis and Xezonakis (2015) also implement survey experiments in Greece, demonstrating that short-term welfare enhancing policies like tax cuts make it more likely that Greek voters will forgive politicians for taking bribes, but opportunities to enter a politician's clientelistic network actually make voters *less* likely to tolerate their corruption.

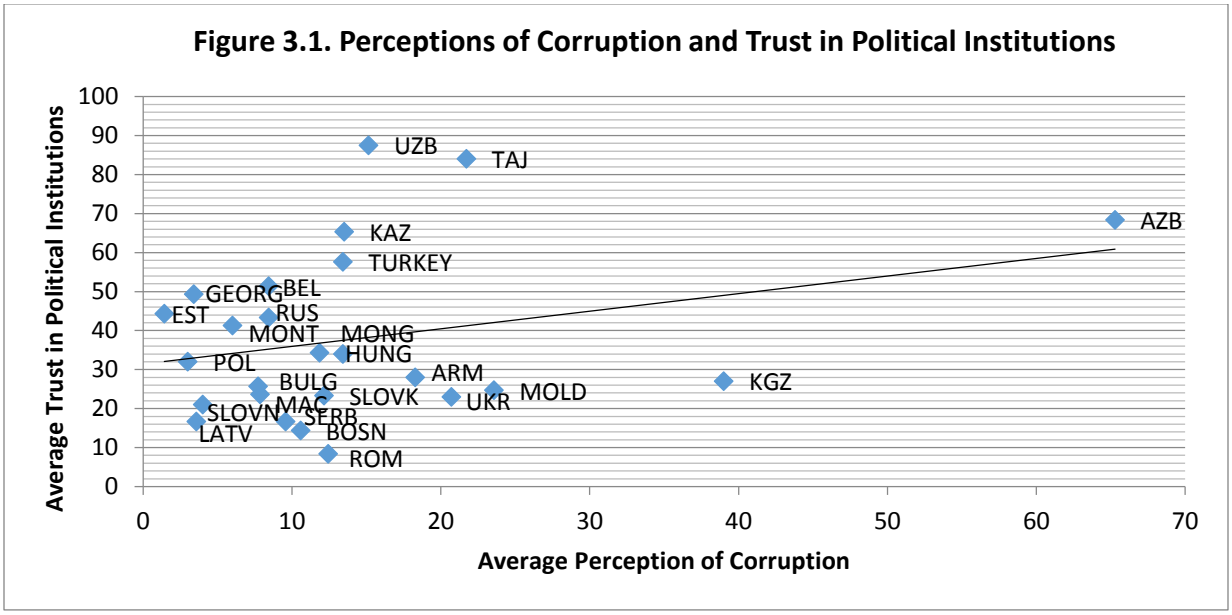
The ability of economic conditions to moderate the political toll of corruption can be observed prevalently but not uniformly in this growing literature. There is sufficient evidence, as Fernández-Vásquez et al. (2014, 3) write, that "the standard economic voting model is indifferent to the legality of the tools available to the politician." Thus, contrary to Rothstein's model of political legitimacy as something driven by procedural fairness and impartiality, it would appear that in a variety of empirical contexts, people care more about results than how one arrives at those results. The outputs of the political process are often more influential on political trust than the way in which the political process is conducted.

3.3 Corruption Tradeoff in Authoritarian Regimes

While there is preliminary evidence that people exchange concerns about corruption for material gains, tests of this theory remain limited primarily to Latin American countries with noted histories of clientelism. This link has not been examined in authoritarian regimes or poorer countries outside Latin America where corruption presents serious challenges to development and political reforms toward the rule of law. I aim to extend a test of this moderation effect in a sample of 30 countries spanning Central and Eastern Europe and all former Soviet states. A preliminary glance at this relationship indicates that people trust political institutions at the same time that they perceive high levels of corruption in everyday life across Central Asia and the Southern Caucasus (Figure 3.1). Contrary to expectations, the relationship between the two variables is not negative

and is noticeably driven upward by seldom studied autocracies like Azerbaijan, Uzbekistan and Kazakhstan. This pattern raises interesting questions about how such countries maintain support even though citizens acknowledge that they must often pay bribes to obtain public services.

4 Figure 3.1 Perceptions of Corruption and Trust in Political Institutions



Data Source: EBRD and World Bank, Life in Transition Survey 2010. Political trust is a sum score of trust in the government, parliament, political parties and local government, scaled 1-100. Corruption is a sum score of perceptions of the necessity of paying bribes across seven public institutions, scaled 1-100 (see Data and Measurement section).

This puzzle differs in some ways from the Latin American-centric literature on corruption tradeoff in the sense that it concerns the concurrence of *petty* (not grand) corruption with high political trust, resembling Rosas and Manzetti's (2015) study on bribe victimization more than the others. This study also eludes the use of straightforwardly reliable dependent variables like intended voting, actual vote shares or incumbent approval, partly due to data limitations and partly due to the nature of studying this link in non-democratic political systems where voting is not a valid carrier of information about political support. Nevertheless, the nature of the puzzle in among the least studied, highly corrupt countries in the world presents an opportunity to examine the

breadth of corruption tradeoff theory with regards to individual-level corruption in different cultures and regime settings.

For theories about moderation effects to work in different contexts, it is important to specify the suspected causal mechanisms. Perhaps the most discussed mechanism in the corruption tradeoff literature concerns clientelist linkages which help citizens overlook corruption in exchange for short-term spoils. Yet governments under different regime settings tend to employ different state capture strategies, not all of which can be considered "clientelist." In Grzymala-Busse's (2008, 644) typology, clientelist states combine political contestation with rent distribution, "exchanging supporter loyalty for rent sharing, as targeted and contingent goods are delivered to select constituencies and individuals." In turn, regimes which fuse party and state tend to distribute rents without allowing for political contestation. Finally, full blown kleptocracies follow strategies of elite predation where the state neither distributes rents nor allows for political contestation. Elites in this scenario expropriate state funds and property for personal use and do not depend on the general population to maintain their offices. They choose to repress rather than redistribute income to citizens. Arguably a number of former Soviet states employ a mixture of fusion and predatory state capture strategies (Russia, Belarus, Kazakhstan, Azerbaijan, Tajikistan), with Uzbekistan employing predatory techniques more exclusively. Based on this picture it would be difficult to test theories about the exchange of corruption concerns for material gains when there are simply no votes for elites to buy, nor material benefits for citizens to receive. Causal mechanisms which work in Latin American clientelist contexts may be absent in authoritarian states.

I would be inclined to argue, however, that forms of clientelism are not limited to democracies. Geddes (2006, 17-18) notes, for instance, that although dictators seek to capture state

rents for personal use, they must nevertheless create stakeholders in the population if they wish to retain elite support and thwart the opposition from taking advantage of popular dissatisfaction with the regime. That is, dictators realize that repression only works so long as people are able to reap some benefits under the system. Failure to meet supermajorities, high turnout or inspire mass rallies even in fundamentally uncompetitive elections can signal an opportunity for elites or underground opposition to remove the incumbent. Elections and parties thus offer incumbents an opportunity to selectively redistribute rents back to the population to secure enough popular support even if they do not directly depend on it for political survival.

Although it seems that public opinion is meaningless in repressive conditions, authoritarian incumbents are themselves often interested in the outcomes of polls when they lack elections to offer consistent signals about the possibility of popular discontent or protest (Treisman 2014b, 372). Hale (2005, 140-141) argues that defecting elites coalesce around mass preferences when they sense an opportunity for regime change. In fact, taking advantage of public opinion is what "crucially distinguishes" cases in which elites have been successful and unsuccessful in ousting authoritarian incumbents across Eurasia in the post-Soviet period (ibid., 144).

Selective redistributive practices are present in a variety of authoritarian regimes. Overland et al. (2009, 179) argue that hydrocarbon-rich governments in particular use natural resources to "buy support, pay off opposition actors and depoliticize society." This argument comes from a longer line of work suggesting that resource rich states which do not depend on tax extraction for political survival can use oil revenues to buy off parts of the population and successfully quell resistance movements (Karl 1997, Ross 2001). Smith (2004) finds that the redistribution of oil rents has a regime stabilizing effect in 107 developing countries over a 40 year period even after controlling for repression. Morrison (2009) confirms this in a sample of over 100 countries over

28 years, interestingly finding that poor dictatorships outspend rich dictatorships on social welfare in order to thwart popular discontent. Wright and Stein (2010) also demonstrate in a sample of authoritarian countries that poor citizens are less likely to hold political institutions accountable for economic performance when they benefit from social spending. These findings tell the broader story of the generalizability of clientelism across regime types: welfare transfers and economic development are priorities even of authoritarian regimes which depend significantly on non-tax revenue.

Broadly, clientelism can be considered a way of compensating for the lack of a functional redistributive state infrastructure. Kitschelt (2000, 873) argues that clientelist politics in Latin America have "constituted the functional equivalent of the welfare state, appeasing the have-nots to abide by political orders that tremendously advantage the haves." In a similar way, elites in post-communist contexts compensate for poor state welfare provision by redistributing resources through citizen-elite linkages when politically opportune. Franke et al. (2009, 127) argue that such linkages are prevalent in autocratic rentier states like Azerbaijan and Kazakhstan, which both combine predatory rent seeking with targeted patronage of interest groups, elites and the general public. Tactics to appease the latter include the provision of free health care, education, higher pensions and subsidized food markets. Between 2003-2007, Kazakhstan experienced real GDP growth rates near 10 percent per year, while Azerbaijan experienced real GDP growth rates between 11 and an astonishing 34 percent in the same period, mainly thanks to foreign direct investments into oil and gas sectors in each country over the previous decade (ibid., 121). Extensive investments went into the construction of lush capitals in both countries, and urban centers experienced increased consumption, employment and public investment rates.

Kendall-Taylor (2012) links rent seeking strategies in these two countries to Geddes' framework, arguing that although buying support from the mass public is not a priority for political survival, incumbents in both countries need it to demonstrate the difficulty of challenges to their authority. Channeling some material benefits back to the public is particularly important to dissuade rivals from believing that they have enough resources and public backing to stage a coup. This would explain why, despite participating in noncompetitive elections, Azerbaijan's President Aliyev commissioned the construction of "five new airports...10 new bridges and 18 underpasses in the capital city of Baku, and 40 new bridges between Baku and the Russian border" in the run up to the 2008 election, and why Kazakhstan's President Nazarbayev spent much more than legally permitted on his 2005 presidential campaign and made overtures like sending trains with medical supplies to provide health care to Kazakhstan's remote regions prior to the election (ibid., 750).

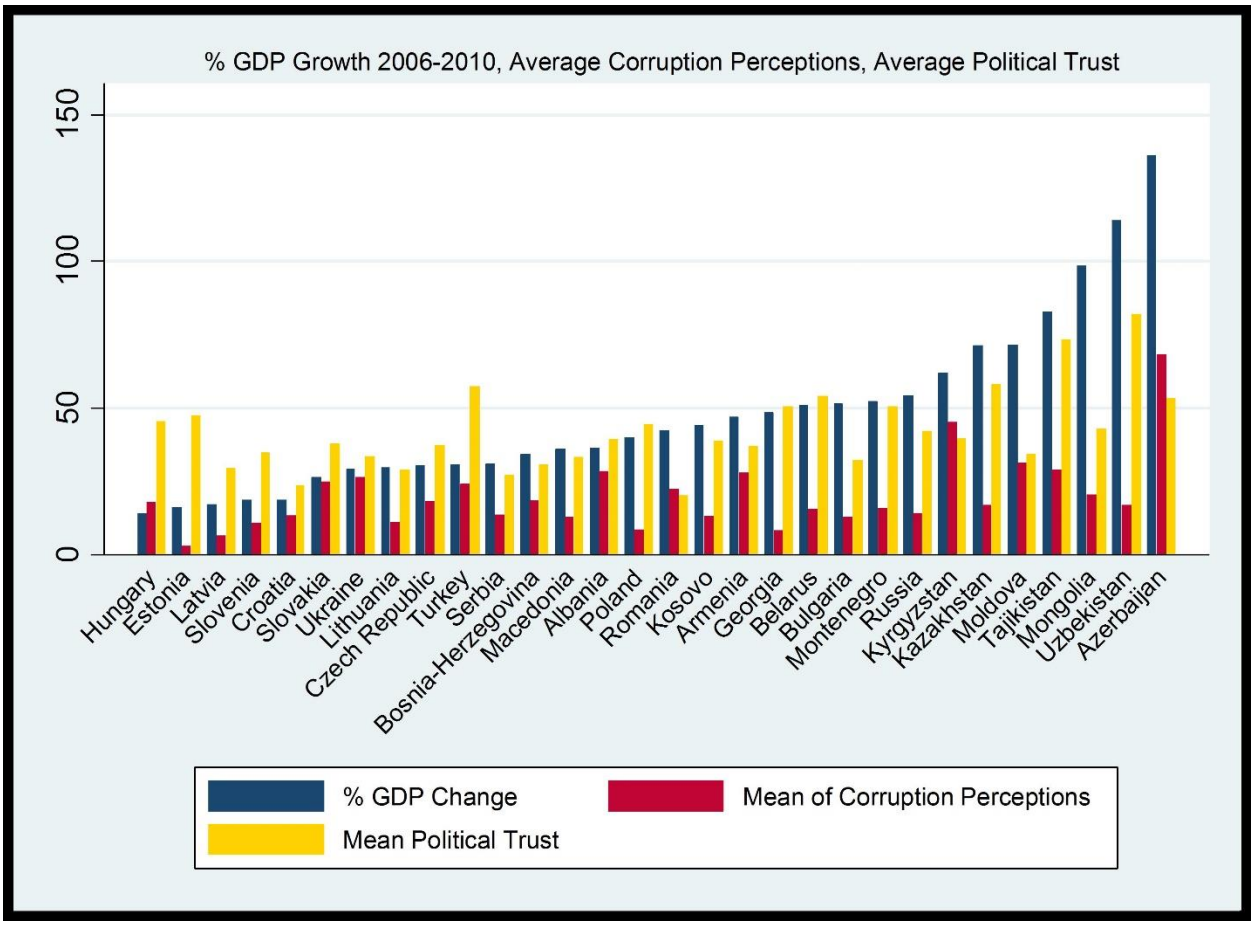
It is particularly popular to argue that economic growth has enhanced the political legitimacy of Vladimir Putin's administration in Russia throughout the early 2000s. Even though Russia has appeared in the bottom third or quarter of Transparency International's Corruption Perceptions Index for the entirety of Putin's tenure as President or Prime Minister, he has never received an approval rating below 60 percent. Treisman (2011, 601) argues that the trajectory of presidential approval across the Yeltsin, Medvedev and Putin presidencies strongly correlates with economic performance indicators, including higher real wages, more job vacancies, lower wage arrears, increases in pensions and drops in unemployment. Such increases in the standard of living are tangibly felt in the population. Fueled by impressive growth, Moscow's municipal government has, for instance, heavily invested in the gentrification of old Soviet parks and exhibition centers into indie art districts and places for western-style cafes and stylish residences, creating a phenomenon some term "hipster Stalinism." In a 2014 Guardian interview, a Moscow student

notes that “after the renovation it became impossible to say that the Department of Culture was corrupt or that our government didn’t care about its people, or that it hated the west and wanted a return to the Soviet Union” (Omidi 2014).

Such trends are consistent with earlier studies showing that economic indicators have significantly more explanatory power over Russian presidential approval than any other factor (McAllister and White 2007, Rose et al. 2004, Mishler and Willerton 2003). In fact, Treisman’s (2014) simulations show that Putin’s tremendous support levels would have likely arisen for any generic Kremlin incumbent enjoying such growth rates even without a war in Chechnya in late 1999, which has traditionally been credited for Putin’s early surge in popularity.

Unsurprisingly, people across the post-Soviet space respond positively to improvements in their wellbeing. In Figure 3.2, the thirty countries across eastern Europe and the post-Soviet space I use in this investigation are ordered by the percent GDP per capita growth in each country between 2006-2010 (in the lead up to the LITS II survey). Countries in which GDP has grown by over 50 percent in this period include Russia, Kyrgyzstan, Kazakhstan, Moldova, Tajikistan, Mongolia, Uzbekistan and Azerbaijan. In the latter two countries, GDP growth more doubled. Countries on this tail of the sample are also the most trusting of government institutions. Political trust tends to rise with growth despite high corruption perceptions in select cases. Is it possible that people are experiencing increases to their material wellbeing to such an extent that the marginal corruption perception becomes less important when they evaluate political institutions?

5 Figure 3.2 Percent GDP Growth 2006-2010, Average Corruption Perceptions, Average Political Trust



With the given evidence, this seems likely. *I hypothesize that corruption perceivers will trust political institutions when they believe they have received gains to their material wellbeing and when they are confident about the performance of the economy in their country*. Testing this theory affords us an opportunity to speculate more broadly about the stability of corrupt governments in different regime contexts. Given an incentive structure in which people regularly trade off corruption concerns for material gains, it is unlikely that we can expect anti-corruption reforms to arise from the top down or from the bottom-up. Short term economic gains can be seen as a sort of opioid for the masses, contributing to developmental reform traps in corrupt societies.

3.4 Regime Moderation

Although corruption tradeoff is plausible in authoritarian regimes, the extent of the tradeoff likely differs across regime types for several reasons. First, marginal corruption perceptions are more likely to reduce political trust in democracies than non-democracies because citizens play a hand in electing politicians and pay for public services in taxes. As a result they are more likely to believe that elected officials are accountable to them and to perceive an instance of corruption as a direct abuse of their trust. Such notions of accountability are less likely to arise in autocracies where citizens do not elect politicians and especially in resource-rich autocracies like Azerbaijan and Kazakhstan which do not principally rely on tax extraction to provide public services. As Huntington (1993, 65) argues in the *Third Wave*, citizens make fewer demands of the government in low tax conditions: "No taxation without representation' was a political demand; 'no representation without taxation' is a political reality."

Beyond notions of accountability, it can also be argued that the mechanism through which the regime type influences corruption perceptions is political repression. Looking only at post-communist democracies in Eastern Europe, Ceka (2012) shows that electoral competition significantly reduces trust in political parties. Despite high quality democratic institutions in the region, electoral competition and vibrant press coverage constantly expose high corruption and depress political trust in populations which had expected much more of democracy after the fall of communism. Sharafutdinova (2010, 160) also finds that even when political competition strives to reduce real corruption, it drives up public corruption perceptions across 40 subnational Russian regions largely due to the use of "black PR," slander and manipulation tactics in political campaigns. Although the authors do not extend their analyses to the full regime spectrum in the post-communist space, their findings could be generalizable: people in autocracies might have

higher political trust because 1) having no real party competition or access to a free press, they simply do not *know* about the extent of corruption, 2) they consume propaganda which paints a heroic picture of anti-corruption efforts by the government or casts blame for poor institutional quality on third parties, or 3) they are aware of the extent of corruption but are too afraid to reveal their true opinion. Any of these reasons can lead us to conclude that citizens of autocracies trust political institutions in the presence of corruption regardless of their economic circumstances or notions of political accountability.

On the other hand, the regime influence can be the exact opposite. Notions of accountability can be present in authoritarian contexts, regardless of repression. Although citizens of autocracies cannot usually hold authorities accountable electorally, they might exercise softer forms of control over political outcomes through public opinion. Treisman (2011, 607) finds that fluctuations in presidential support across illiberal democracies and semi-authoritarian states "may mirror economic volatility- and the efforts of rational voters, against the odds, to hold their leaders to account" (ibid.). Mishler and Willerton (2003, 117) and McAllister and White (2008, 948) posit that Russians are even more likely to hold the government accountable for economic performance than citizens of Western democracies. Since political accountability in authoritarian contexts is concentrated in a leader with extensive discretion over resource distribution, it is easier for a citizen to place blame for poor economic performance than it would be in the context of democracy, where accountability is more distributed across institutions and a broader global market.

Using a cross-regime sample in Asia, for example, Huhe and Tang (2016) find evidence that economically dissatisfied strata of the population trust political institutions less in autocracies than in democracies because citizens of autocracies do not distinguish between leaders and the regime itself, holding the entire system accountable when things go wrong. Citizens of

democracies, in contrast, tend to penalize only incumbents rather than the system, making democratic political institutions more resistant to economic downturns. It could be alternatively hypothesized that even in the absence of functioning electoral controls or a free press, citizens of autocracies can be just as (if not more) punitive of corruption as citizens of democracies. In this view, they are fully equipped with liberal notions of political accountability one would find in democracies; the only reason they do not demand representation is simply that they *cannot*, or, at least, not without a threat to their life or liberty.

To sum, *I hypothesize that citizens of autocracies who observe more corruption will trust political institutions more than citizens of democracies who observe more corruption, either due to lacking notions of accountability or repression*. The alternative hypothesis is that citizens of authoritarian regimes will trust political institutions even less in the presence of corruption than those living in democracies because political institutions can be more directly perceived as responsible for the provision of public goods. Looking at the regime context helps us learn not only about how all forms of government might supply economic gains to overcome corruption resentment in the public, but how the institutional rules of the game moderate this corruption tradeoff. Some political systems might be less vulnerable to anti-corruption challenges than others.

3.5 Post-Materialist Values and Education

One might argue that my proposed hypothesis is too economically deterministic. Inglehart and Welzel (2005) observe that while citizens of industrializing societies tend to privilege gains in material wellbeing, they eventually begin to focus on goals beyond immediate survival. After experiencing growing living standards, people seek more control over their lives and will eventually no longer be willing to exchange political rights for material gains. The state cannot easily coax or control independently resourced and independently minded pockets of society. We

can hypothesize on these grounds that economic sentiment and development spur precisely the opposite of corruption tradeoff. If people adopt post-material values as they become wealthier, every marginal unit of material benefit will offer diminishing returns, while every marginal unit of corruption will become more costly. Greater satisfaction with material wellbeing and increasing wealth might lead citizens to impose an even greater penalty on political institutions for corruption rather than reduce it.

Part of the shift toward post-materialist values comes from greater education. For Inglehart and Welzel, as societies develop economically, beneficiaries of higher education become more intellectually autonomous and creative, no longer needing to rely on the guidance of authorities or rigid routines to make decisions. People become more skeptical of hierarchies and dogmas as they gradually become emancipated from authority (ibid., 26-29). Authority itself comes to be seen as something which resides within individuals rather than rule-making institutions. Infractions against individual rights and liberties thus become increasingly morally troublesome for people with more education.

Evidence from developing countries suggests that more educated people tend to be more informed and critical of political affairs and thus less inclined to trust government (Catterberg and Moreno 2005; Chang and Chu 2006; Luhiste 2006; Cho and Kirwin 2007; Lavallée et al. 2008; Arnold et al. 2012) although education tends to increase political trust in Norway (Christensen and Laegrid 2005). This slight nuance is upheld in other studies which suggest that people with more education have less political trust in highly corrupt societies, but more political trust in cleaner societies (Tverdova 2011; Hakhverdian and Mayne 2012).

Hakhverdian and Mayne (2012) posit that education impacts institutional evaluations in two ways. First, education makes it easier for people to acquire and process information about institutional performance: more highly educated people tend to be more interested in politics, read more newspapers and participate in civil or political organizations. Glaeser et al. (2007) show that education strongly predicts transitions from authoritarianism to democracy and helps consolidate democracy over the long term by increasing the number of beneficiaries of political participation. They find evidence in favor of the hypothesis that education socializes people in a way that supports civic mindedness, public service and political engagement. Kitschelt (2000, 857) argues that educated citizens tend to develop longer time horizons to observe the outcomes of their political choices and are no longer willing to organize around immediate material incentives: "within countries, a rising urban white-collar and professional middle class is the first to defect from clientelism."

In the context of the former Soviet states, there is some speculation about a post-material value shift in Russian society as part of the explanation for the 2011-2012 anti-Putin protest cycle. Treisman (2014b) argues that mass discontent was largely a matter of disappointment with economic circumstances among provincial urbanite, female and rich strata of the population. Members of the creative class (dwellers of Moscow or St Petersburg with higher education and a laptop computer) did lose faith in the president, but made up only three percent of the population. This makes it implausible that the greater population experienced a shift in values. On the other hand, Robertson (2013, 21) argues that there is a broader qualitative difference in Russian protest activity between the early 1990s and the later 2000s: "citizens are increasingly, it seems, willing or able to make connections between material issues and more abstract issues like civil rights or election fraud."

If people are indeed experiencing a shift towards post-materialist values across the sampled territory, I expect that my tradeoff hypothesis will not be supported. There will be evidence that as corruption perceivers become better off, they will trust political institutions less. This effect will be more pronounced in more educated strata of the population. In other words, *my hypothesis is that less educated corruption perceivers will trust political institutions more than highly educated corruption perceivers.*

3.6 Culture

One might object to the rational choice approach inherent in corruption tradeoff theory as something which dismisses the influence of culture on the development of attitudes toward corruption and political institutions. Culture is often seen as a form of path dependence. Prominent historian Richard Pipes (2004, 10) has argued that Russia's history of authoritarianism, absence of respect for private property and lack of a civil rights tradition has led Russians to "desire strong tsarist rule" and made Russians, "even in modern times, the least socialized or politicized people on the European continent." Mishler and Willerton (2003, 114-116) review theories arguing that Russian citizens tend to prefer strong leadership and have "collectivist, passionately nationalistic" or "rabidly anti-Western" values. Such cultural path dependence would make Russia an inappropriate subject of research. Notwithstanding the complex set of variables that determine political orientations, in *some* ways, Russians are immune to economic incentives, or fall into a special category of behavior because of their national character.

This is *not* a particularly useful way of discussing cultural influence. First, Russians are by no means the lone subjects in this kind of theorizing. Although theories have been advanced about the authority deferential tendencies of Asian and Arab societies, research shows almost uniformly that people in these regions respond rationally to institutional performance (Chang and Chu 2006;

Kim and Voorhees 2011; Maseland and Hoorn 2011; Tessler et al. 2012; Yang and Tang 2010). Secondly, the cultural footprint is more often than not an artifact of citizens' perceptions of economic performance or material wellbeing. Although Kim and Voorhees (2011, 418) make a point about the trust-promoting effects of Confucian attitudes, it appears that the ultimate source of that political trust is economic performance and not a cultural tendency to sacrifice individual wellbeing for group wellbeing: although the distribution of state rents to favored companies would be "regarded as corrupt behavior in transparent societies…citizens [in East Asia] perceive such ties to be beneficial to their country's economic growth." Similarly, although Askvik and Ishtiaq (2013, 472) argue that Bangladeshis have a different, perhaps non-Western, understanding of institutional trustworthiness, it too boils down to economic performance: "despite a number of deficiencies, people tend to trust public and political institutions because such institutions have performed well and contributed to economic progress and better living conditions during recent years."

The same trajectory of the argument appears in studies of the former Soviet space. In his anthropological study of corruption and patronage in Azerbaijan, Barrett (2015) summarizes Rogers' (2006) account of the Soviet concept of the leader as a "khozyain," or a "master, owner, administrator, boss, man of the house" whose competence is measured by his "ability to maintain patronage networks, barter goods and favours, and honour moral expectations that he would provide for his employees and the community at large" (Barrett 2015, 525). Barrett argues that this notion largely defines how citizens relate to President Aliyev in modern day Azerbaijan. It is understood by citizens that the "khozyain" can steal from the common resource pool of the country as long as he redistributes back some of the rents. The compatibility of corruption and political trust in Azerbaijan and other countries in my sample could thus be explained by specific notions

of leadership arising from the post-Soviet moral economy. Again, however, even this specific cultural notion is based on the leader's ability to bestow or redistribute material benefits back to citizens. In the Azeri case, the president has managed to prove his managerial competence by declaring amnesties on utility debts and directly gifting free gas to households (ibid., 527). It is likely, therefore, that evaluations of economic conditions and material wellbeing moderate the link between corruption perceptions and political trust across both regime types and cultures.

This is not to argue that culture plays no role, but that many cultural theories can often be more coherently rephrased in a rational choice framework. It is important to be careful about making overly general claims about culture ("X people have authoritarian values") as such theories have no limits over the range of empirical phenomena they can claim to explain and are therefore extremely difficult to falsify. Because culture is difficult to define or measure, practically no amount of empirical testing can deny its presence or influence. To account for culture, we should avoid 1) treating it as a form of path dependence which reduces the subjects of study to exceptional cases or aberrations, 2) making overgeneralizations or universal causal claims and 3) arguing for its influence without specifying the mechanisms through which it operates. If we are unable to avoid these temptations, we need not deny culture's potential influence, but we are also not in a good position to use it as a meaningful explanation of an empirical puzzle.

Perhaps one way we can think of culture in a meaningful way is by leaning on a game-theoretical literature which discusses how people solve cooperation games given repeated defection. In this sense, culture can come into play as something which regulates norms of behavior through a lifetime of signposts and learning trials (Ostrom 1998, 10). Corruption can be considered a cultural norm or "social equilibrium with low quality exchanges" in which all parties to a transaction assume noncompliance with formal rules (Villoria et al. 2013, 4; Gambetta and Origgi

2009). If cheating behavior goes unsanctioned, rule breaking becomes the norm and transaction partners develop new shared expectations of cooperation over time. High corruption perceptions at the household level can produce "moral externalities" by desensitizing individuals to the costs of corruption and making it a normal part of life or the country's "culture" (Sharafutdinova 2010, 149). This desensitization or normalization is hard to detect empirically, but is certainly a plausible scenario if petty corruption perceptions prove not to exert any influence on a person's political trust regardless of economic circumstances, regime type or education.

3.7 Theory and Measurement

3.7.1 Political trust

In this study I use the Life in Transition Survey II (LITS II) produced by the European Bank for Reconstruction and Development (EBRD) and the World Bank. The cross-sectional sample from late 2010 surveys almost 39,000 households in 35 countries to assess public attitudes on a range of social, political and economic variables.[6] Two –stage clustered, stratified sampling was employed across regions in each country.[7] I analyze thirty countries including all of Eastern Europe and the hard to reach former Soviet states across Central Asia. I omit the five Western European countries thus far used in Chapter 2 because perceived bribery across public institutions is practically nonexistent in these cases, yielding very large margins of error around the key

[6] Specifically, the sample I use includes (with number of respondents interviewed in each country in parentheses) Albania (1029), Armenia (953), Azerbaijan (988), Belarus (895), Bosnia (1075), Bulgaria (1007), Croatia (997), Czech Republic (1007), Estonia (989), Georgia (959), Hungary (1031), Kazakhstan (943), Kosovo (1083), Kyrgyzstan (992), Latvia (1004), Lithuania (1003), Macedonia (1060), Moldova (1023), Mongolia (981), Montenegro (970), Poland (1587), Romania (1068), Russia (1550), Serbia (1506), Slovakia (995), Slovenia (988), Tajikistan (996), Turkey (1004), Ukraine (1547), Uzbekistan (1417)

[7] See the full methodological report on the EBRD website: http://www.ebrd.com/news/publications/special-reports/life-in-transition-survey-ii.html

corruption-trust link. The results of this 30-country study remain robust to their inclusion. To measure political trust, I use the standard LITS question on political trust analyzed in Chapter 2.

There are theoretical limitations to this measurement of political trust. The literature on corruption tradeoff uses a range of dependent variables, including political trust, approval ratings of incumbents and vote shares. It is far from obvious that one can derive the same theoretical conclusions from each of these measures. After all, I may distrust the system but support my favored candidate to manipulate the system ahead of someone else. People ultimately might not give political institutions a pass on corruption, but will do so for benefactor-incumbents who spread the wealth. Barrett (2014) argues that this is likely the case in Azerbaijan.

In this way institutional trust and incumbent support might also imply different logics of corruption tradeoff. One might be willing to vote for the incumbent in good times despite corruption, but it makes less sense to say that one will be willing to *trust institutions* in good times despite corruption. This latter disposition toward institutions does not quite reflect the same tradeoff mechanism. Perhaps one way of describing the potential difference is to think about the *salience* of political institutions. As one sees more corruption in daily life, it is not necessarily the case that she is *willing to trust institutions in good times* as much as she has *no reason to disapprove of political institutions in good times*. Political institutions appear more salient to people who are exposed to corruption in bad times, and less salient in good times. Incumbent politicians, however, do not merely fluctuate in salience through the ups and downs of the economy; they are perhaps trusted in something closer to the individual-level conception of the term "trust." Despite corruption, people are still willing to vote for the incumbent so that he may continue to deliver material prosperity. This form of corruption tradeoff is tied to a conscious decision to support someone in return for the performance of a concrete task. The diffuse support

that institutional trust represents does not imply an immediate target of accountability in the same way that specific support for an incumbent does. For this reason we might expect that the interaction between corruption, economic performance and *institutional trust* will be weaker than that between corruption, economic performance and *incumbent approval*.

Nevertheless, the verdict is less clear on how this plays out in authoritarian regimes, where people are less likely to distinguish between the system and leader largely because the leader has *become* the system (Huntington 1993, 50). Marien (2012, 15) has argued on empirical grounds that incumbent and system-level trust are hard to disentangle. Even in democracies, it is rare that one evaluates political institutions completely independently from who is in power. Because it is hard to tell which of these scenarios applies in the present empirical setting, I will test theories of corruption tradeoff against both alternatives. To consider incumbent support, I will cross-check my results using trust in the presidency as a dependent variable in the ten countries in my sample with presidential or semi-presidential systems. The presidency is not a clear measure of the president himself in these countries, but it is the closest approximation available in the survey to incumbent support.

3.7.2 Corruption

To measure corruption, I create an additive index of a question gaging perceptions of petty corruption: "In your opinion, how often do people like you have to make unofficial payments or gifts in these situations?" a) Interacting with road police; b) Requesting official documents; c) Going to courts for a civil matter, d) Receiving public education, e) Receiving medical treatment in public health system, f) Requesting unemployment benefits; and g) Requesting other social security benefits. This measure differs from other corruption measures in the political trust literature, which consist either of perceptions of grand corruption, corruption scandals, reports of

bribe victimization, or survey experimental vignettes in which respondents are presented with potential scenarios of elected officials' misdemeanors. Once again, it is not clear that the same theoretical conclusions should be drawn about corruption tradeoff across this range of variable choices.

While corruption measured in scandals or perceptions of officials' misdemeanors is fairly straightforwardly tied to incumbent approval or institutional trust, it is less clear that perceptions of petty or bureaucratic corruption are tied to those outcome variables. Why, after all, should people hold political institutions accountable for their encounters with bribery, and why would they be willing to overlook this petty corruption when the economy is doing well? In the post-Soviet context, there is a clear link between local bureaucracies which interact with people and the federal-level decision-making bodies which allocate resources for the public services they provide. In Russia, for example, public goods spending on policing, courts, medicine and education largely comes from the federal budget (Libman 2012, 1325). According to a 2013 survey in Russia, Dmitriev (2015, 240) finds that up to 78 percent of respondents across the country would address their economic grievances to the President or the federal government over local authorities. Treisman (2014b, 19) finds that Russian citizens' beliefs that hospitals and clinics have deteriorated is the strongest predictor of low incumbent approval over citizens' beliefs about performance in other public service sectors. This effect becomes insignificant after controlling for perceptions of the economy, showing that corruption and economic perceptions are correlated. It remains plausible that economic perceptions will moderate the extent to which citizens hold institutions accountable for that corruption.

Unfortunately I am unable to conduct a robustness check using perceptions of grand corruption because they are unavailable in the LITS survey. But even if these data were available,

they are not an optimal means of testing the present theory. In their well cited study of corruption tradeoff in 19 Latin American countries, Zechmeister and Zizumbo-Colunga (2013, from here out abbreviated as "ZZC") measure corruption perceptions with the following survey question in the 2010 AmericasBarometer (part of the Latin American Political Opinion Project, or LAPOP): "Taking into account your own experience or what you have heard, corruption among public officials is: very common, common, uncommon or very uncommon?" Similar approaches are taken in numerous studies using survey data to study the trust-corruption relationship.[8] In theory, this approach straightforwardly matches up the questions in a way that citizens are evaluating the corruption and trustworthiness of the same political institutions or individuals, providing for a clean, logical interpretation of corruption tradeoff above and beyond what citizens' experiences of bribes can deliver. In practice, however, the interpretation is not so straightforward.

We can take ZZC's study as an example. They study corruption tradeoff across 19 Latin American countries and the United States. Off the bat, the correlation between country means of corruption perceptions in LAPOP and the 2010 Corruption Perceptions Index (CPI) is -.432 (we expect a negative relationship because the CPI ranks higher corruption in descending, rather than ascending order). The relationship is only moderately strong, suggesting that the LAPOP data is tapping into something slightly different than the CPI. In LAPOP, The US has significantly lower grand corruption perceptions (.702) than the Latin American mean (.731) although the US perceptions are higher than Uruguay (.625), El Salvador (.649), Brazil (.667), Nicaragua (.678) and Chile (.685). This trend may appear odd at first glance, but it generally follows expert

[8] Perceptions data are usually measured with a version of a question like "how widespread do you think corruption and bribe taking are in your national government/local government?" (Asian Barometer), "how widespread do you think bribe taking and corruption is in this country? (Gallup World Poll, New Democracies Barometer) or "how many of the following people do you think are involved in corruption?" with choices such as the president, MPs, national government officials, police, teachers, judges, military officers, political parties, NGO leaders, etc. (Afrobarometer, Americas Barometer).

evaluations of corruption in the same year: each of these countries but Nicaragua has the cleanest CPI scores in the region, although Chile and the USA score considerably higher than some of the other top five (Chile CPI=7.2, USA CPI=7.1, Uruguay CPI =6.9, Costa Rica CPI=5.3, Brazil CPI=3.7). Among the most vivid discrepancies, Nicaragua scores in the top five cleanest countries out of the 21 according to its mean grand corruption perception score in LAPOP, even though it scores in the bottom five of the CPI (with a score of 2.5). Also puzzlingly, Costa Rica appears in the bottom five countries by its grand corruption perception mean in LAPOP while it scores in the top five cleanest countries of the region in the CPI just behind Chile, the US and Uruguay (CPI score of 5.3). Roughly speaking, if we were to use the CPI as a benchmark of "true" corruption as judged by academic and business elites, it would appear that Costa Ricans highly overestimate the amount of corruption in their country while Nicaraguans highly underestimate it. US respondents' perceptions of grand corruption are about five percent higher than those of Brazilians, even though Brazil is 63 percent more corrupt than the US according to the CPI. What are we measuring, after all? Which scale is more believable as a measure of corruption?

In their own study using a similar question, Manzetti and Wilson (2007, 958) argue that the only thing that matters is people's points of view: "we respect the survey respondents' own definition of corruption… if a respondent indicates that there is a high level of corruption in his or her country, we assume that this reflects particular acts that the respondent has witnessed or otherwise acquired information about and deems corrupt." After all, if we are interested in learning about beliefs, survey responses must already implicitly contain the normative criteria used to form those beliefs. But this isn't entirely right. We might understand the kinds of practices one imagines by the term "corruption" in a single place at a single time, but if we stretch the survey question thinner over time and space, it will begin to capture a wider range of phenomena which may tap

into notions we are unable to disentangle or interpret. Supposing we find evidence of a tradeoff effect using this measure in a pooled country sample, we might at best conclude that on average people trade off concerns about *something they don't like* for economic gains. The corruption perceptions measure can become a net which catches everything anyone finds wrong with the political system. These evaluations are benchmarked against different national histories and institutional practices which are hard to pinpoint with cross national survey data. Citizens of country A might not be willing to overlook the same kinds of political transgressions as citizens of country B in good economic times, yet such transgressions might fall under the umbrella of "corruption" for respondents in both countries. Granting zoning rights to favored construction companies might be more serious for some, and massive money laundering might be more serious for others. Some governments will get away with more than others. It is not clear just how serious the pooled tradeoff effect really is. Adding country fixed effects into an econometric model to soak up country-level characteristics (as ZZC do) does not solve the comparability problem because we still would not understand the context and implications of tradeoff. For this reason, survey experiments which present respondents with detailed corruption vignettes such as those run by Winters and Weitz-Shaprio (2013) offer much more interpretable tests of the theory of corruption tradeoff, although they are costly to implement in a large number of countries and difficult to make representative of the populations from which they are sampled. Conceptual shallowness is often the price one pays for widely representative cross-national data. Still, survey-based corruption measures are not all equally shallow; I will argue below that petty corruption perceptions are more cross-nationally interpretable than grand corruption perceptions.

Before proceeding, however, I will consider another reason why petty corruption perceptions can be problematic for a study of corruption tradeoff. While corruption perceptions

can reduce political trust, low political trust can also magnify the amount of corruption one sees. Mainly to avoid endogeneity, several authors prefer predictors which capture experiences, rather than perceptions of corruption (Cho and Kirwin 2007; Clausen 2011; Seligson 2002). These survey questions inquire whether and how much respondents have had to pay bribes to obtain public services in the recent past. Presumably, these actual instances of bribery are less predictable by one's political trust levels. Razafindrakoto and Roubaud (2010, 1058) go as far as to argue that corruption experiences provide an *objective* and *scientific* approach to measurement. Perceptions data, they claim, are validated on soft justifications such as correlations with expected outcomes and correlations between indices; the "match between perception and fact" is based "more on belief than on scientific proof." Their solution is to use respondents' self-reports of the frequency and amount of bribes paid to government officials in eight African countries (including authoritarian regimes). They defend this measure with methods they themselves criticize, however, citing the experience-based measures' expected correlations, as well as "the respondents' considerable interest in governance issues, the interviewers' impressions concerning data collection, the low non-response rates and the internal consistency of the data," none of which they substantiate with evidence (ibid., 1062). Their ultimate finding is that only 13 percent of the eight-country sample has experienced bribe victimization in the previous year. The authors conclude that country experts who perceived the rate to be 52 percent, particularly those in favor of free market principles (who could be upset that the countries have failed to adopt their favored policies), provide unreliable information about "the objective phenomenon" of corruption (ibid., 1067).

Although these perspectives carry small grains of truth, a hard turn to experience-based data should not exaggerate the benefits of such measures. First, while actual instances of extortion or bribery are not functions of one's political trust, self-reports of such instances most definitely

can be. If I am upset with the government, I can overstate how many bribes I was forced to pay last year. Moreover, what respondents decide to admit in a survey interview is related to their own unobservable characteristics, which, despite our inability to model them, influence both trust and corruption responses. Using experience-based data offers no more *proof* of causality than a perception-based measure, and is likely *more* prone to non-response and social desirability biases, particularly in contexts where admission of bribery can result in legal or criminal proceedings. Azfar and Murrell (2009), for instance, show that reticence among survey respondents (whether attributable to giving knowingly false answers, saying 'don't know' or refusing to answer) significantly lowers estimates of informal corruption, making the measures incomparable across populations where reticence varies. A measure of experienced bribery is simply not more effective at demonstrating causality, nor is it more scientifically or objectively representative of "actual" corruption than a perception-based measure. In the absence of proper instruments for corruption perceptions, regression coefficients using either measure should not be interpreted as causal. This applies to the current study as well.

Perceptions of petty bribery do not deliver a causal interpretation, but I would argue that they still have certain advantages for a cross-national study of corruption tradeoff. First, perceptions of petty corruption among "people like me" are likely to be more truthful than personal experiences of petty corruption because they do not require an admission of complicity in bribery. Second, perceptions of petty corruption do not depend on the respondent having used a public service in the previous year. Not owning a car does not mean one is not aware of how commonly traffic police demand bribes of motorists. Third, unlike perceptions of grand corruption in the federal government, petty corruption perceptions are not as sensitive to corruption scandals, media exposure or partisanship. Klasnja et al. (2014), for instance, find that grand corruption perceptions

vary significantly based on these variables while reports of bribe victimization remain stable over time. This stable picture of corruption is particularly important for citizens of authoritarian states: propaganda might easily sway perceptions of grand corruption, but it has less power over perceptions of bribery when one uses the police or hospital. Fourth, petty corruption perceptions are likely more equivalent in meaning across societies. Bribery imposes a cost on individuals wherever they are located, exposing them to real institutional dysfunction and the mismanagement of public resources. A lot less is up to the imagination than in cases of grand corruption or scandals, which can be perceived differently in different societies. I am not aware of conceptual differences inherent in perceptions of bribery between public institutions like schools, hospitals and others, but will consider potentially systematic differences in their response patterns across the sampled countries in the next chapter.

3.7.3 Economic Context

In the political trust literature, satisfaction with material wellbeing is often conceptualized along several dimensions, most commonly sociotropic evaluations (concerning the national economy), pocketbook evaluations (concerning one's personal finances), retrospective evaluations (the financial situation today relative to the past) and prospective evaluations (the financial situation today relative to what one expects in the future). Less used but perhaps no less interesting are measures of general life satisfaction, which can capture a wider sense of a person's health, wealth and capabilities that the other measures cannot. While these indicators are immensely important – and usually powerful predictors of political trust— authors often depend on one or two of these survey questions to draw broad empirical conclusions, neglecting robustness checks to alternative measures of wellbeing. To improve our ability to infer meaningful attitudinal relationships from survey data, it is important to exercise caution on several fronts.

While theoretically important, measures of the above economic sentiments can add noise to the regression of trust on corruption. Unavoidably, all three perception-based measures (trust, corruption and economic evaluations) can be influenced by unobserved features of the individual, leading to bias in the parameter estimation of key variables. Simultaneous and reverse causation is also probably in play: high satisfaction with one's financial situation might simultaneously increase political trust and lead one to perceive less corruption. For this reason, it is important to test models of the tradeoff effect using both subjective and more objective (income based) measures of wellbeing.

Subjective and objective indicators are likely to produce different outcomes for statistical reasons. In the same way that subjective heat rankings are a "noisy proxy" for real temperatures (Westfall and Yarkoni 2016, 3), subjective evaluations of economic conditions are noisy proxies for actual economic conditions. Using an imperfect proxy to estimate the effect of the true construct has been shown to inflate Type I errors in simulation studies (ibid., 5). In other words, measurement unreliability in the proxy often leads the researcher to conclude that it has a significant effect when it does not. This concern is less pertinent when we are explicitly interested in belief concepts rather than using the beliefs as proxies for "real" concepts. Nevertheless, we should expect differences in regression coefficients between subjective and objective economic indicators.

Subjective and objective economic indicators should also be treated distinctly on theoretical grounds. In the context of economic development in the post-Soviet states, it is reasonable to suspect that the poorer strata of the population will feel the benefits of growth more substantially (and thus appear more financially satisfied) than the rich. A marginal increase in the standard of living will be more significant for someone who has less to begin with. It could be the

case that wealthier people trade off concerns about corruption because they are materially better off. But if poorer strata are more satisfied with their financial situations, we might observe that they trade off corruption more than the rich. After all, relying on short term benefits, the worst off stand to benefit the most from social transfers and economic growth (Kitschelt 2000, 857).

Measures of economic sentiment are no less prone to errors and non-equivalence across populations than any other attitudinal measure. Some populations might over- or underestimate their wellbeing due to cultural differences, question wording, translations and other effects of the survey instrument. Deaton (2013, 51) documents that Scandinavian countries tend to score exceptionally highly on subjective measures of wellbeing, Latin American countries relatively highly, East Asian countries rather poorly, and former Soviet countries exceptionally poorly: "We do not know whether these continental differences come from genuine differences in some objective aspect of wellbeing, from national differences in disposition, or from national differences in the way people respond to the ladder question" (the ladder question asks respondents to rank their life satisfaction in "steps" from 0 to 10).

At the moment there does not appear to be a straightforward way to ensure the equivalence of economic perceptions. Evaluations of the national economy are perhaps more plausibly related to evaluations of political institutions – and it could be this measure (rather than an evaluation of personal wellbeing) which captures corruption tradeoff. Retrospective evaluations of the economy, meanwhile, do not always lead to the same conclusions as hopes for the future (Lewis-Beck and Stegmaier 2008, 312). Conflating conceptually distinct phenomena like these will likely obfuscate the meaning of any resulting regression coefficient. For clarity of interpretation, I choose to test the trust-corruption relationship for moderation against each indicator of wellbeing and economic sentiment separately. While this does not fully alleviate measurement equivalence concerns, the

robustness checks can alert us to unexpected cross-country variation in economic sentiment reporting.

The Life in Transition Survey allows us to exploit a rich set of questions covering more objective measures of wellbeing like monthly consumption of food, utilities and transportation. Such data are beneficial because they give us an idea of a person's capabilities to lead a good life regardless of what she feels about her circumstances. If we assume that growth has a real impact on human welfare, we can use these measures to identify the beneficiaries of the economic growth or spoils distribution at the household level. Helpfully, such indicators of welfare enhancement are not entirely dependent on characteristics of individuals which directly influence their political trust and corruption perceptions. Such "objective" measures are, of course, also vulnerable to measurement error, particularly in shadow economies. The sheer newness of data collection practices on poverty in developing and authoritarian countries should make us particularly wary of the reliability of such data.

Despite their interesting uses, these more objective data might do a poor job of capturing actual beneficiaries of government welfare transfers or economic growth. It is unclear if the level of objective material wellbeing has resulted from government actions which would lead individuals to trust political institutions in the presence of corruption. The comparatively wealthier members of society may have simply been better off for generations, and the poorer strata of society who have been experiencing gains to their wellbeing from recent macroeconomic conditions haven't yet risen on these objective wellbeing indicators in a cross-sectional snapshot. Thus, current material wealth may prove inconsequential for one's tendency to trade off corruption concerns. In the absence of longitudinal data about how households have fared financially over time, the next best thing is economic sentiments, which can capture a respondent's consideration

of improvement or deterioration over time, bringing us back to square one. Using attitudinal data can be worth the potential estimation uncertainty because of its theoretical relevance: knowing how people *feel* is essential to the study of political trust and legitimacy, perhaps more so than knowing people's more objective material circumstances.

There are costs to internal validity whatever measures one chooses. In practice, the bar is not high to begin with, as measures of economic development and sentiment are often assumed to be perfectly measured in the trust literature. To move beyond this precarious assumption, we should take advantage of methodological advances to detect and control for measurement error. Where this is not possible, robustness checks to alternative measures are crucial for conducting meaningful data analysis.

3.8 Theoretical Summary

In over a decade of cross-national political trust research, it has become clear that corruption tends to reduce political trust and economic satisfaction tends to enhance it. Yet we still do not know enough about how these variables interact. This study contributes most importantly to the literature by considering the possibility that people trade off concerns about corruption in good economic times across a diverse regime spectrum. Although introducing different regime types complicates the theory of corruption tradeoff, it is a fruitful complication, allowing us to test the generalizability of a theory in environments with dissimilar rent seeking arrangements. Great theories explain a lot with a little. In seeking to explain more, "the persuasiveness of a theory depends not only on how many facts are explained, but also on how diverse are the kinds of facts explained" (Olson 1982, 13). If we begin to ask why leaders get re-elected despite corruption in clientelist democracies, it is not a far step to ask why political institutions maintain support despite corruption of different varieties across different regime types and cultures. Indeed, it would be odd

to suggest that rational tradeoffs of corruption concerns for material gains are only a Latin American phenomenon, or, alternatively, that people of select cultures are immune to such incentive structures. If commonalities exist across many societies, we will better understand why governments with poorly functioning institutions tend to succeed in both democratic and non-democratic environments. Such an analysis can help illuminate why some societies get stuck in the process of development. In what follows, I will provide an empirical test of the theory of corruption tradeoff in thirty countries spanning Eastern Europe and the former Soviet space.

4 Chapter 4: A Revision of the Theory of Corruption Tradeoff

4.1 Introduction[9]

In this chapter, I will provide an empirical test of the theory of corruption tradeoff outlined in Chapter 3. Before proceeding, I will restate my key hypotheses.

H1: Corruption perceivers will trust political institutions when they believe they have received gains to their material wellbeing and when they are confident about the performance of the economy in their country.

H2: Corruption perceivers in autocracies will trust political institutions more than their counterparts in democracies, either due to lacking notions of accountability or repression.

H3: Less educated corruption perceivers will trust political institutions more than highly educated corruption perceivers.

4.2 Data and Variables

In the following section I will elaborate on the variable choices I discussed in Chapter 3. To reiterate my data source, I use the Life in Transition Survey II (LITS II) produced by the European Bank for Reconstruction and Development (EBRD) and the World Bank. The cross-sectional sample from late 2010 surveys almost 39,000 households in 35 countries to assess public attitudes on a range of social, political and economic variables.[10] Two –stage clustered, stratified

[9] Supplementary materials for this chapter are available in Appendix B.

[10] Specifically, the sample I use includes (with number of respondents interviewed in each country in parentheses) Albania (1029), Armenia (953), Azerbaijan (988), Belarus (895), Bosnia (1075), Bulgaria (1007), Croatia (997), Czech Republic (1007), Estonia (989), Georgia (959), Hungary (1031), Kazakhstan (943), Kosovo (1083), Kyrgyzstan (992), Latvia (1004), Lithuania (1003), Macedonia (1060), Moldova (1023), Mongolia (981), Montenegro (970), Poland (1587), Romania (1068), Russia (1550), Serbia (1506), Slovakia (995), Slovenia (988), Tajikistan (996), Turkey (1004), Ukraine (1547), Uzbekistan (1417)

sampling was employed across regions in each country.[11] I analyze thirty countries including all of Eastern Europe and the hard to reach former Soviet states across Central Asia.

4.2.1 Coding for Political Trust

To measure political trust, I use the standard LITS question on political trust: "To what extent do you trust the following institutions?" Response choices range from complete distrust (1) to complete trust (5). For preliminary models, I construct my dependent variable as a simple sum score of trust in the government/cabinet of ministers, parliament, local government and political parties. I rescaled this 5-point measure to run from 0-100 for ease of interpretation. I ran a multiple group confirmatory factor analysis to gain confidence about the cross-country measurement equivalence of this combination of indicators in Chapter 2, and will presently run a robustness check in a SEM framework using the latent factor of political trust rather than the sum score. To consider incumbent support, I will cross-check my results using trust in the presidency as a dependent variable only in the ten countries in my sample with presidential or semi-presidential systems (that is, where the president exercises real, not symbolic power).[12] The presidency is not a clear measure of the president himself in these countries, but it is the closest approximation available in the survey to incumbent support. Finally, I cross-check my results against a measure of political trust in which I omit the 'trust in the local government' indicator to limit the analysis to federal level political institutions. This simpler measure is also equivalent in meaning across this set of countries (see Chapter 2).

[11] See the full methodological report on the EBRD website: http://www.ebrd.com/news/publications/special-reports/life-in-transition-survey-ii.html

[12] These countries are Armenia, Azerbaijan, Belarus, Georgia, Kazakhstan, Lithuania, Romania, Russia, Tajikistan and Ukraine. Uzbekistan could not be included due to omission of the presidential trust question.

4.2.2 Coding for Corruption

To measure corruption, I create an additive index of a question gaging perceptions of petty corruption: "In your opinion, how often do people like you have to make unofficial payments or gifts in these situations?" a) Interacting with road police; b) Requesting official documents; c) Going to courts for a civil matter, d) Receiving public education, e) Receiving medical treatment in public health system, f) Requesting unemployment benefits; and g) Requesting other social security benefits. I ran a preliminary exploratory factor analysis in each country to assess possible inconsistencies in response patterns to this set of indicators. All indicators overwhelmingly load on a single factor, suggesting that people have a general awareness of corruption in society which informs their views of bribe taking across specific institutions. No alternative factor showed strong enough loadings to justify investigating conceptually separate dimensions of petty corruption perceptions across countries.

This index is heavily right skewed, with a mean of 19.4 points out of 100. To better observe its effects across its range, I split the variable into terciles of corruption ('1' referring to the subset of the sample which did not perceive any necessity of bribery, '2' referring to the subset with low-medium corruption perceptions, and '3' referring the subset with high corruption perceptions).[13] I run my analysis both with this categorical variable and with a simpler dummy variable in which '1' refers to the top tercile (the third of the sample with the highest corruption perceptions) and '0' refers to the bottom two terciles (the two-thirds of the sample with mild or moderate corruption

13 Approximately one third (34.54 percent) of the sample recorded no corruption perceptions. This made splitting the corruption perceptions scale into thirds more intuitive, allowing us to better isolate the highest third of corruption perceivers at the right tail of the distribution from the "no corruption" and "low/medium corruption" terciles of the distribution.

perceptions). Because the results do not substantively differ between the two, I present my results using this simpler dummy variable.

4.2.3 Coding for Economic Context, Regime Type and Controls

I use four main measures of economic context. To measure pocketbook evaluations, I use the question asking respondents to agree or disagree (on a 5-point scale) with the statement "I am satisfied with my financial situation as a whole." I measure sociotropic assessments of the economy with a question asking respondents to agree or disagree with the statement "on the whole, I am satisfied with the present state of the economy." Finally, although there are no data about income, I proxy personal wealth with a question about average monthly consumption: "approximately how much does your household spend on each of these items per month? a) food, beverages and tobacco, b) utilities (electricity, water, gas, heating, fixed line phone), and c) transportation (public transportation, fuel for car). Instead of considering a continuous measure of consumption, I split the measure into consumption quartiles within each country to examine non-linearities in the trust-corruption relationship across levels of consumption. Finally, I use a more subjective measure of income, which I refer to as the "ladder question": "Please imagine a ten-step ladder where on the bottom, the first step, stand the poorest 10% of people in our country, and on the highest step, the tenth, stand the richest 10 percent of people in our country. On which step of the ten is your household today?" I split the ten steps into five to ease interpretation. To help balance out observations in each category, my categorical variable marks "1" for those who placed themselves on steps 1 or 2 (13.96 percent of respondents), '2' on steps 3 or 4 (37.23 percent of respondents), '3' on step 5 (27.91 percent of respondents), '4' on steps 6 or 7 (17.63 percent of respondents) and '5' on steps 8 and above (3.28 percent of respondents). I complement these

measures with robustness checks using measures of life satisfaction, retrospective and prospective evaluations of the economy.[14]

To measure regime effects, I use Polity Scores from 2010, creating three categories of autocracy, anocracy and democracy. My sample contains four autocracies (Azerbaijan, Belarus, Kazakhstan, Uzbekistan), five anocracies (Russia, Kyrgyzstan, Tajikistan, Bosnia, Armenia) and 21 democracies. Polity scores can be criticized for their atheoretical content, particularly the notion of 'anocracy' which falls vaguely in the middle of the regime spectrum. In the current sample, this middle category captures several semi-authoritarian states and maps broadly onto 2010 Freedom House scores. By looking at all three categories, we can assess the extent to which the middle category differs from the most repressive countries. If at least some political freedom results in different assessments of corruption, we will have more room to manoeuver with theory building about which characteristics of regimes affect public opinion. Despite bones we might pick with the middle category, the classification of autocracies and democracies in this sample is not controversial according available regime indices.

Before proceeding with the analysis, I checked for the potential clustering of high corruption perceptions in the least economically satisfied strata of the sample. I found, however, that there is no great variation in levels of corruption perceptions across levels of satisfaction with personal finances (at sat=1, mean corruption is 18.59 out of 100, at sat=2, mean corruption is 19.297, at sat=3, mean corruption is 20.218 and at sat=4, mean corruption is 19.654). Corruption perceptions are also relatively evenly distributed by levels of satisfaction with the national economy and consumption quartiles. Corruption perceptions are, predictably, *not* evenly

[14] See Appendix B for the full set of robustness checks.

distributed by regime type: in autocracies, average corruption is 28.64, in anocracies it is 25.84 and in democracies it is 16.29. This is not a great danger to the analysis but deserves acknowledgement. If we find that corruption exerts less influence on political trust in democracies, we cannot conclude that this is simply because there is less corruption in democracies. In the following analysis, the effects of corruption are assessed by comparing the subset of the sample which perceives the highest level of corruption in both democracies and autocracies. Respondents in the "high corruption" category should be exposed to similar levels of petty corruption, wherever they are located. We *would* be in some danger if perceptions of bribery meant different things to respondents in different regime types, or if reticence has prevented open or honest responses in authoritarian regimes. Although I have argued that this is unlikely to be the case with petty corruption perceptions, it remains an empirical question that deserves future study and recommends some interpretive caution in the present one.

4.3 Models and Robustness Checks

I begin my analysis with a set of country fixed effects OLS models in which I estimate a set of interactions between corruption and economic context, and between corruption and regime type. Alongside a vector of sociodemographic controls, I also include interactions between corruption and education to determine whether more highly educated corruption perceivers place higher penalties on political institutions than less educated corruption perceivers.[15] Education is

[15] Summarizing the variables of interest, the main specification is:

$$\text{Political Trust}_{ij} = \beta_0 + \beta_1 \text{Corr}_{ij} + \beta_2 \text{Econ}_{ij} + \beta_3 \text{Regime}_j + \beta_4 \text{Corr}_{ij} \text{XEcon}_{ij} + \beta_5 \text{Corr}_{ij} \text{XRegime}_j + \beta_{6-36} \text{CountryDummies}_j + \alpha_i + u$$

where "Corr" is corruption, "Econ" is a measure of economic context or sentiment (such as sociotropic or pocketbook evaluations, consumption quartiles or the ladder question) and "Regime" measures regime type. α_i represents a vector of individual level controls which include age, age squared, household size, urban and capital dwellers, education and its interaction with corruption.

split into three categories to separate those who have obtained at most primary, secondary and post-secondary level educations. While I include all interactions together, I re-ran my analysis with each interaction separately, ensuring that the interpretation presented here is accurate. Given the categorical nature of my independent variables, this OLS model is similar to ANOVA, the simplest and most parsimonious test of the theory. I test this model for robustness against other economic variables (mentioned above) and model specifications, the results of which are available in Appendix B.

With regards to alternative specifications, I test for robustness against a set of two-level models in which the effect of corruption perceptions on trust varies across the 30 countries. Doing so removes the unrealistic assumption in the OLS models that corruption has a fixed influence on political trust across countries and corrects for spatial autocorrelation among individuals clustered in the same territory. The intra-class correlation (ICC) indicates that approximately 28 percent of variation in the trust-corruption relationship is attributable to country-level characteristics. A likelihood ratio test confirms that a multilevel structure is appropriate ($p<0.000$), justifying the random coefficient specification. I also check the original OLS specifications in a single and two-level structural equations (SEM) framework in which I substitute the political trust sum score with a metrically invariant latent factor based on the same indicators (trust in the government, parliament, local government and political parties). This specification controls for the portion of the political trust latent factor which cannot explain variation among the chosen indicators. As discussed in Chapter 2, implementing such "controls" helps us separate actual political trust levels from noise associated with their cross-country measurement.[16] I also include robustness checks against OLS models with standard error adjustments for survey weights. Because the results do

[16] See Westfall and Yarkoni (2016)

not substantively differ between these specifications, I present only the fixed effects OLS models below and leave the results of the robustness checks in Appendix B.

4.4 Results

A baseline country-fixed effects model with no interactions (Table 4.1) shows that there is a very strong correlation between economic sentiment and political trust. People who are very satisfied with their personal finances (scoring 4 or 5 on a scale of 1-5) trust political institutions 19-21 points more (on a scale of 1-100) than those who are very unsatisfied (scoring 1). People who are very satisfied with the national economy trust political institutions by 25-26 points more than those who are very unsatisfied. These steep effects are highly significant and robust across all model specifications.

Table 4.1 Baseline OLS with country fixed effects, no interactions

(DV= Political Trust sum score, scaled 1-100, 30 countries)

	Satisfied with financial situation	Satisfied with national economy	Consumption Quartiles	Ladder Question
High Corruption	-2.953*** (0.334)	-2.462*** (0.325)	-3.640*** (0.373)	-3.504*** (0.341)
Economic Context (see column heading)				
2	8.295*** (0.460)	10.052*** (0.396)	-0.689 (0.471)	5.301*** (0.491)
3	14.143*** (0.471)	19.754*** (0.451)	-0.491 (0.488)	9.154*** (0.518)
4	19.671*** (0.494)	26.007*** (0.505)	-2.073*** (0.522)	13.336*** (0.570)
5	21.287*** (0.864)	25.173*** (0.923)	N/A	16.266*** (0.917)
Anocracy	-11.022*** (1.284)	-9.996*** (1.240)	-12.065*** (1.471)	-11.435*** (1.305)
Democracy	-44.001*** (1.131)	-41.394*** (1.105)	-44.595*** (1.269)	-46.945*** (1.159)
Middle Ed	0.246 (0.392)	1.187*** (0.379)	1.220*** (0.437)	0.212 (0.400)
Upper Ed	0.114 (0.412)	1.689*** (0.397)	2.075*** (0.460)	0.281 (0.423)
R^2	0.323	0.359	0.270	0.289
N	23313	23253	20383	23538

Note: Controlled for female, age, age squared, household size, urban and capital dwellers (see Appendix B2 for full table). Standard errors in parentheses. *p<0.1, **p<0.05, ***p <0.01

It appears that sociotropic perceptions of the national economy have a slightly stronger effect on political trust than assessments of personal wellbeing or financial satisfaction, which is consistent with the literature. A slightly less pronounced effect is evident in the ladder question: people who perceive themselves at least above average in wealth trust political institutions 13-16 points more than those on the bottom rungs of the ladder. A measure of consumption quartiles, the least subjective indicator, does not show any trust-promoting effect. People in the top consumption quartile of their country tend to trust political institutions by two points *less* than those in the bottom quartile. This negative effect tends to vary in significance and remains substantively negligible across different model specifications.

On average, people who perceive the most amount of corruption trust political institutions less by 3-5 points than those who perceive little to no corruption. Does the extraordinary economic stimulus to political trust wipe out this corrosive influence of corruption? It appears that the answer is overwhelmingly no (Table 4.2). It is not the case in almost any model that satisfaction with personal finances, the national economy, life satisfaction, prospective or retrospective economic hope makes individuals less punitive of corruption. The same goes for income status: people in higher consumption quartiles or on higher steps of the ladder are not more tolerant of corruption than those who consume less or believe they are less well off. Almost all interaction coefficients across all model specifications are both substantively and statistically insignificant.

9 Table 4.2 OLS with country fixed effects

(DV= Political trust sum score, scaled 1-100, 30 countries)

	Satisfied with financial situation	Satisfied with national economy	Consumption Quartiles	Ladder Question
High Corruption	-6.047*** (1.402)	-5.026*** (1.377)	-7.054*** (1.443)	-8.441*** (1.489)
Economic Context (see column heading)				
2	8.875*** (0.559)	10.183*** (0.484)	-0.396 (0.568)	4.995*** (0.588)
3	14.441*** (0.574)	20.289*** (0.545)	-0.276 (0.588)	8.839*** (0.622)
4	19.006*** (0.598)	25.543*** (0.613)	-2.000*** (0.627)	13.296*** (0.688)
5	21.586*** (1.030)	25.761*** (1.121)	N/A	15.365*** (1.148)
	Corr x Sat. with	Corr x Sat. with	Corr x consumption	Corr x ladder
Corr x 1 (Ref)	financial situation	national economy	quartiles	
Corr x 2	**-1.836* (0.970)**	**-0.373 (0.822)**	**-1.009 (0.989)**	**0.943 (1.056)**
Corr x 3	**-0.966 (0.975)**	**-1.630* (0.918)**	**-0.776 (0.986)**	**0.928 (1.094)**
Corr x 4	**1.821* (1.014)**	**1.172 (0.996)**	**-0.409 (1.000)**	**0.112 (1.176)**
Corr x 5	**-1.238 (1.851)**	**-1.880 (1.911)**	**N/A**	**2.419 (1.865)**
Ref=Autocracy				
Anocracy	-12.707*** (1.369)	-11.758*** (1.329)	-14.113*** (1.581)	-13.237*** (1.391)
Democracy	-45.395*** (1.175)	-42.514*** (1.155)	-45.693*** (1.316)	-48.317*** (1.201)
Corr x Anoc	**4.873*** (1.305)**	**4.813*** (1.290)**	**5.801*** (1.495)**	**5.868*** (1.335)**
Corr x Democ	**4.407*** (1.118)**	**3.608*** (1.121)**	**4.145*** (1.262)**	**5.035*** (1.146)**
Middle Ed	0.270 (0.466)	1.299*** (0.452)	1.162** (0.522)	0.248 (0.476)
Upper Ed	0.585 (0.484)	2.063*** (0.467)	2.164*** (0.544)	0.672 (0.496)
Mid. Ed x Corr	-0.155 (0.793)	-0.439 (0.769)	0.125 (0.884)	-0.188 (0.812)
Upper Ed x Corr	-1.309* (0.784)	-1.107 (0.760)	-0.180 (0.878)	-1.068 (0.803)
R^2	0.324	0.361	0.271	0.289
N	23313	23253	20383	23538

Note: The 1-5 column indicates survey responses from 1 to 5 on each economic sentiment variable, consumption quartile or step of the ladder (see column headings) "High corruption" is a dummy variable where 1 indicates the top tercile of the corruption perceptions distribution and 0 indicates the bottom two terciles (those who perceive little to no corruption). This dummy variable is interacted with each category of economic context and regime type. Key interaction effects are in bold. Controlled for female, age, age squared, household size, urban and capital dwellers (see Appendix B3 for full table). Standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01

In fact, a few models hint at the opposite effect. There is some evidence that people who express at least some satisfaction with their financial situation are *more* punitive of corruption than those who express no satisfaction with their financial situation. This is particularly evident if we examine *presidential*, rather than general political trust in the subset of 10 semi or fully presidential countries in the sample (Table 4.3). High corruption perceivers (that is, those in the top tercile of the corruption perceptions distribution) who score a 4 out of 5 on satisfaction with personal finances trust the presidency 4.2 points (out of 100) less than high corruption perceivers who are least satisfied with their personal finances. High corruption perceivers who are maximally satisfied (scoring a 5 out of 5) with personal finances trust the presidency 9.1 points less than high corruption perceivers who are least satisfied. The same trend appears for sociotropic economic sentiments: high corruption perceivers who score a 4 out of 5 on satisfaction with the national economy trust the presidency 4.3 points less than high corruption perceivers who are least satisfied, and high corruption perceivers who score a 5 out of 5 on satisfaction trust the presidency 12.4 points less than high corruption perceivers who are least satisfied. High corruption perceivers are no different from their 'low or no' corruption perceivers at low levels of economic satisfaction, but at higher levels of both pocketbook and sociotropic economic satisfaction (scores of 4 or 5), high corruption perceivers are significantly less trusting of the presidency than 'low or no' corruption perceivers. From this we can infer that the political toll of petty corruption gets heavier, not weaker, as economic sentiments improve. We do not observe this trend for consumption quartiles or the ladder question. Ultimately, even if dictators are credited with growing the economy or handing out occasional spoils, they are not only *not* given a pass for petty corruption among people satisfied with their (or their country's) economic situation, they appear to be held even closer to account.

Within this analysis of trust in the presidency, we also observe different conditioning effects at the macro and micro levels of economic evaluation. National economic conditions appear to be a benchmark against which citizens judge presidential performance more readily than personal economic conditions. If one's country is experiencing high levels of economic growth, one presumably begins to demand more of the president; the national context can inspire blame attribution for corruption on the national leader. With a weaker interaction coefficient, perceptions of household economic performance do not translate into presidential penalties quite as easily. From this finding, one could posit that household income is not a critical contributor to institutional and presidential trust. Indeed, one could go further in hypothesizing that higher *income* corruption perceivers should be *less* likely to punish the presidency because they are better positioned to remove themselves from the state by seeking private governance solutions and avoiding contact with petty corruption altogether. Lower income corruption perceivers would in turn be expected to *increase* their presidential penalty because they depend more on state service provision and are more likely to suffer from petty corruption. The results in Table 4.3 show, however, that income indicators (both proxies for consumption and the ladder question) do not moderate the corruption-trust link. Citizens are more likely to impose penalties on political authorities in their trust assessments because they *believe* something about their economic conditions, not because they are experiencing a certain economic reality. This finding depends on an endogenous relationship between three attitudinal variables and certainly requires interpretive caution. At the same time, we have reason to believe that relatively objective income indicators have little to do with corruption perceivers' orientations toward the state.

10 **Table 4.3 Presidential Trust, 10 countries, OLS with country fixed effects**

(DV= Trust in the presidency, scaled 1-100)

	Satisfied with financial situation	Satisfied with national economy	Consumption Quartiles	Ladder Question
High Corruption	-1.696 (1.516)	-0.248 (1.428)	-2.952** (1.469)	-1.558 (1.608)
Economic Context (see column heading)				
2	10.942*** (1.171)	12.871*** (1.139)	1.203 (1.251)	5.741*** (1.149)
3	16.648*** (1.234)	22.473*** (1.261)	-0.082 (1.316)	11.903*** (1.279)
4	22.656*** (1.339)	29.604*** (1.384)	-0.485 (1.398)	13.963*** (1.528)
5	23.618*** (2.723)	30.030*** (3.043)	N/A	14.718*** (3.363)
Ref= Corr x 1	Corr x Sat. with financial situation	Corr x Sat. with national economy	Corr x consumption quartiles	Corr x ladder
Corr x 2	**-1.909 (1.878)**	**-1.352 (1.777)**	**-2.220 (1.976)**	**-1.892 (1.866)**
Corr x 3	**-1.181 (1.941)**	**-3.032 (1.915)**	**0.810 (1.985)**	**-4.121** (2.013)**
Corr x 4	**-4.243** (2.053)**	**-4.321** (2.012)**	**0.549 (2.012)**	**-3.430 (2.301)**
Corr x 5	**-9.078** (4.263)**	**-12.379*** (4.301)**	**N/A**	**-2.271 (4.728)**
Anocracy	-30.384*** (1.568)	-29.715*** (1.549)	-34.878*** (1.754)	-32.839*** (1.573)
Democracy	-37.483*** (1.337)	-33.425*** (1.344)	-41.377*** (1.443)	-39.099*** (1.357)
Middle Ed	1.759* (0.968)	2.448*** (0.954)	2.442** (1.067)	1.623* (0.979)
Upper Ed	-0.487 (0.941)	0.713 (0.928)	0.735 (1.045)	-0.499 (0.956)
R^2	0.415	0.438	0.391	0.392
N	8048	7931	6955	8140

Note: The 1-5 column indicates survey responses from 1 to 5 on each economic sentiment variable, consumption quartile or step of the ladder (see column headings) "High corruption" is a dummy variable where 1 indicates the top tercile of the corruption perceptions distribution and 0 indicates the bottom two terciles (those who perceive little to no corruption). This dummy variable is interacted with each category of economic context. Key interaction effects are in bold. Controlled for female, age, age squared, household size, urban and capital dwellers (see Appendix B5 for full table). Standard errors in parentheses. *p< 0.1, **p<0.05, ***p<0.01

Returning to general political trust in the full sample, I decided to test if the tradeoff effect might be contextual. When evaluating political institutions, individuals most likely do not judge their material conditions in a vacuum; they are inevitably influenced by how they are doing relative to others. If the town has generally fallen into an economic slump and everyone around appears to be suffering, one's personal financial circumstances might matter less for the way she evaluates

corruption and political institutions. Furthermore, politicians deliver spoils or economic growth to regions rather than individuals. To assess this contextual effect, I looked at how local economic conditions might moderate high corruption perceivers' penalties on political institutions by breaking the sample into 444 subnational regions with an average of 54 people per region. A variance components analysis indicates that there is significant variation in the trust-corruption relationship across these regions. Political trust responses between two people in the same region have a .34 correlation, whereas responses between two people in the same country but different regions have a .24 correlation. Local context appears to matter. I tested whether people's tendency to reduce political trust for corruption depends on the overall proportion of people who are very satisfied with their personal finances or the economy, or the proportion of people who rank themselves on top of the ladder *in their locality*. Once again, however, the interaction effects are all negligible and insignificant for each variable and across the range of the variable (Table 4.4). While good economic conditions and optimism at home consistently boost political trust, people who perceive high levels of corruption do not reduce their penalty on political institutions when their locality appears to be prospering.

11 **Table 4.4 Three-Level Analysis, Contextual Effects**

DV=Political Trust scaled 1-100, 30 countries

	Model 1: Regional Proportion Most Satisfied with Personal Finances	Model 2: Regional Proportion Most Satisfied with National Economy	Model 3: Regional Proportion on Steps 6-10 of the Ladder
High Corruption	-6.282*** (2.121)	-3.788* (2.213)	-6.848*** (1.982)
Economic Context (see column heading)			
2	7.776*** (0.452)	9.489*** (0.393)	4.759*** (0.481)

3	13.241*** (0.465)	18.618*** (0.451)	8.576*** (0.511)
4	18.754*** (0.492)	24.760*** (0.507)	12.206*** (0.569)
5	20.391*** (0.851)	24.029*** (0.916)	15.462*** (0.909)
Regional Proportion	4.201 (3.747)	9.932** (3.947)	4.142 (3.494)
Corr x Regional Proportion	**2.193 (3.631)**	**-2.039 (3.628)**	**1.714 (3.672)**
Middle Ed	-0.296 (0.456)	0.671 (0.444)	-0.317 (0.466)
Upper Ed	0.308 (0.478)	1.747*** (0.463)	0.536 (0.489)
Mid. Ed x Corr	-0.623 (0.794)	-0.879 (0.775)	-0.527 (0.809)
Upper Ed x Corr	-1.834** (0.807)	-1.771** (0.788)	-1.703** (0.824)
Anocracy	-18.698*** (6.192)	-13.313*** (5.020)	-19.901*** (7.010)
Democracy	-24.722*** (5.005)	-17.463*** (4.117)	-27.134*** (5.655)
Corr x Anoc	**5.405** (2.239)**	**4.518** (2.275)**	**5.792** (2.342)**
Corr x Democ	**3.796** (1.765)**	**2.599 (1.920)**	**4.072** (1.798)**
ICC (at region)	0.23	0.18	0.26
N	23313	23253	23538

Note: In this three-level analysis, there are 30 countries and 444 subnational regions with an average of 52.5 observations per region. The 'regional proportion' variable calculates the proportion of individuals in each region who scored a 4 or 5 (out of 5) on satisfaction with personal finances or the national economy, or steps 6-10 (out of 10) on the income ladder. A variance components analysis indicates that there is significant variation in the trust-corruption relationship on both country and regional levels. The intra-class correlation (ICC) of political trust responses at the region level is .34 (or the correlation of responses between two people in the same region). The regional ICC in this table indicates the correlation between two people after accounting for region-level proportions of economically satisfied strata in the sample. Controlled for female, age, age squared, household size, urban and capital dwellers (see Appendix B10 for full table). Standard errors in parentheses. *p< 0.1, **p<0.05, ***p<0.01

What role does the regime type play? Firstly, it should be noted that dwellers of democracies are consistently and dramatically *less* trusting of political institutions than dwellers of autocracies. In most country fixed effects models, this difference in political trust between the regime types hovers around an impressive 45 (out of 100) points. In models without country dummies, the difference remains near 25 points. Even more surprising, the regime type plays a significant moderating role in the way high corruption perceivers penalize political institutions, but not quite in the way I hypothesized. Across all models, dwellers of democracies or anocracies

who perceive a high level of corruption are significantly *more*, not less trusting of political institutions than dwellers of autocracies. This is among the most significant and stable outcomes across all model specifications, with and without country dummies. Looking at the variance components, the regime effect tends to cut down variation in the trust-corruption relationship across countries by more than half. This moderating effect is modest, but consistent and significant, venturing above a one percent significance level only in a couple of regressions. In almost all models, high corruption perceivers in autocracies trust political institutions between 4-6 points (out of 100) less than high corruption perceivers in democracies and anocracies.

What can we say about the ambiguous territory of the "anocracy" which contains Russia, Kyrgyzstan, Tajikistan, Bosnia and Armenia? On the whole, these countries do not behave entirely like democracies or full-fledged autocracies, which somewhat vindicates their separation from the others. On average, citizens of these countries tend to trust the presidency less than citizens of autocracies by 30-34 points out of 100, a similar magnitude of difference as that between democracies and autocracies. Their evaluations of political institutions soften: citizens of anocracies trust political institutions less than citizens of autocracies, but only by 12-14 points. Semi-authoritarian leaders receive decidedly less support from their populations than fully authoritarian leaders. We should pause, however, in concluding that autocracies elicit high trust responses through repression. As shown above, citizens of autocracies who register widespread corruption report less trust in government than high-corruption perceivers in freer societies.

Does education have a moderating role in the trust-corruption relationship? It appears not. Across all models and specifications, education has almost nothing to do with political trust or the way people penalize political institutions for corruption. The effect of education is consistently close to zero and usually statistically insignificant. This null effect holds when I look at a simple

baseline with no interactions and a model with only education interactions. Part of the reason might be the influence of state propaganda in schools on the authoritarian part of the regime spectrum. The trust-mobilizing tendencies of these educational institutions might be canceling out the trust-eroding tendencies of those in more open political systems. This possibility remains to be explored in the future.

4.5 Discussion

In this study, I hypothesized 1) that high corruption perceivers in the former Soviet space will trust political institutions more when they are experiencing a flourishing economy or improvement in material conditions and 2) that high corruption perceivers in autocracies will trust their political institutions more than their counterparts in democracies. I found evidence for neither hypothesis. Despite the extraordinary influence of economic sentiment and material wellbeing on political trust, people who perceive more corruption in society *do not* reduce penalties on political institutions in good economic times. If anything, they *enhance* penalties as they get better off, particularly when evaluating the presidency. Similarly, despite the fact that citizens of autocracies are on average much more trusting of political institutions than citizens of democracies, high corruption perceivers in autocracies consistently place higher penalties on political institutions than high corruption perceivers in democracies. How can we explain these unexpected outcomes? The results of each hypothesis test have separate implications for the theory of corruption tradeoff and for the study of authoritarian political legitimacy. I will take each in turn.

4.5.1 Implications for the Theory of Corruption Tradeoff

It would be misleading to suggest that these results are entirely at odds with the theory of corruption tradeoff prevalently supported in Latin American clientelist democracies. I was able to obtain replication materials for Zechmeister and Zizumbo-Colunga's (2013) study of corruption

tradeoff in 19 Latin American presidential systems to see whether the results would change if I changed the locus of corruption. The authors premise their study on a measure asking survey respondents whether they think "corruption among public officials" is common or uncommon. As I argued before, it is difficult to understand the meaning of their strong positive interaction coefficients given the lack of clarity in the meaning of the corruption measure across countries. But supposing the authors *are* correct—that people across Latin America who believe there is a good deal of "corruption among public officials" *are* prone reduce penalties on their presidents in good economic times—do people respond the same way if they have encountered a great deal of petty corruption? I replicated ZZC's study, changing the corruption measure to one of three petty corruption measures: 1) *government bribe victimization* measured by the question "In the last twelve months, did any government employee ask you for a bribe?" This is a dummy variable, 1 for "yes" (5.48 percent of respondents) and 0 for "no"; 2) *local bribe victimization*, a dummy variable indicating 1 for respondents who reported that they were asked to pay a bribe either at school, hospital or work in the last 12 months (49.07 percent have and 50.93 have not); and 3) *high local bribe victimization*, a dummy variable indicating 1 for respondents who reported being asked to pay a bribe in all three of the above places (school, hospital and work) in the last 12 months (7.18 percent of respondents).

Consistent with ZZC's findings, there is no evidence of a significant interaction effect between measures of petty corruption and pocketbook evaluations of the economy (Table 4.5). Bribe victims of the government or local bureaucracies across Latin America neither increase nor decrease their presidential approval as they become more satisfied with their personal finances.

12 **Table 4.5 Zechmeister and Zizumbo-Colunga (2013) Replication**

	ZZC Fully Interacted Model	My replication with govbribe	My replication with locbribe	My replication with highbribe
Corruption (see column heading)	-11.52*** (1.62)	-4.54** (1.84)	4.22*** (1.06)	-1.01 (1.45)
Pocketbook Eval	5.86 (2.39)	8.29*** (0.89)	7.97*** (1.27)	8.13*** (0.89)
Sociotropic Eval	18.06*** (0.203)	26.76*** (0.84)	31.49*** (1.17)	27.13*** (0.86)
Corr x Pocketbook	**2.80 (2.98)**	**2.17 (3.28)**	**0.78 (1.64)**	**5.62** (2.82)**
Corr x Sociotropic	**11.17*** (2.58)**	**2.88 (2.93)**	**-9.23*** (1.49)**	**-2.37 (2.33)**
Voted for Incumb	12.10*** (0.353)	12.66*** (0.35)	12.61*** (0.35)	12.64*** (0.35)
Income	-4.22*** (0.84)	-4.42*** (0.80)	-4.63*** (0.79)	-4.62*** (0.79)
Age	-1.61*** (0.491)	-0.44*** (0.09)	-0.42*** (0.09)	-0.43*** (0.09)
Female	1.00*** (0.276)	0.97*** (0.26)	1.06*** (0.26)	1.02*** (0.25)
Education	-2.39*** (0.679)	-3.22*** (0.67)	-3.23*** (0.67)	-3.38*** (0.67)
Urban	-0.37 (0.448)	-0.68 (0.45)	-0.73 (0.44)	-0.73 (0.45)
R Sq.	.28	0.28	0.28	0.28
N	27311	28464	28594	28594

Note: Standard errors in parentheses. *p< 0.1, **p<0.05, ***p<0.01. DV= Presidential approval. *Govbribe*= government bribe victimization, *locbribe*= local bribe victimization, *highbribe*= high local bribe victimization (see p. 128 for variable descriptions). In this replication, ZZC get a significant positive interaction between grand corruption and sociotropic economic evaluations which helps mitigate the negative effect of corruption on presidential approval. We expect to see similarly positive interactions with measures of petty corruption in the replication columns but do not observe them. A full table with interactions across the range of both pocketbook and sociotropic evaluations is available in Appendix B11.

There *is* a significant positive interaction coefficient between high bribe victimization and pocketbook economic evaluations, but the upward climb of the interaction term across levels of pocketbook satisfaction is not significant at any level of satisfaction (Appendix B11). *Not* consistent with ZZC's findings but consistent with my own, there is a significant *negative* interaction between local bribe victimization and sociotropic economic evaluations. This means that bribe victims become *more* critical of the president as they become more satisfied with the national economy. To understand the full extent of this decreasing trust, I looked at the interaction across the range of economic satisfaction (Appendix B11). Bribe victims who score a 2 out of 5 on satisfaction with the national economy approve of the president 3.5 points less than bribe

victims who are least satisfied. Bribe victims who score a 3 out of 5 approve 6.02 points less than the least satisfied, bribe victims who score a 4 out of 5 approve 6.8 points less than the least satisfied, and bribe victims who score a 5 out of 5 on satisfaction approve 12.37 points less than the least satisfied. Each of these differences in approval among bribe victims at different levels of satisfaction is significant at the 0.01 level. A similar negative interaction effect is observable for victims of high bribe victimization (those who had to pay a bribe at school, work *and* hospital), but because there are fewer observations on this variable, the standard errors are much bigger at high levels of economic satisfaction, making it difficult to infer the real interaction estimate in the population. Bribe victims of the government in general (a vaguer question which does not inquire about what sort of government interaction took place) are neither more approving nor disapproving of the president at higher levels of economic satisfaction.

On the whole, this is consistent with my current analysis: good economic times do not buoy presidential approval among corruption perceivers. Perhaps the most interesting addendum to ZZC's findings is that even if people are more likely to ignore *grand corruption* in good times, they are actually likely to become more concerned about *petty corruption*. That is, they might forgive politicians for scandals or misuses of public funds (although precisely what constitutes corruption across Latin American countries remains unclear) but may also become more displeased with the institutional decay at the bottom. One possible reason for this finding is that grand corruption might be capable of producing economic gains for people in a clientelist system, whereas corruption at the bottom of the system which strips people of personal resources only becomes more repugnant as they get better off.

But what exact mechanism accounts for this repeated and theoretically surprising presidential penalty at high levels of economic satisfaction? While data limitations prevent a

comprehensive investigation of this question, the LITS survey does afford us an opportunity to test preliminary explanations. One possible reason for this finding is that economically satisfied corruption perceivers are more likely to hold the presidency accountable because they can increasingly *afford* to develop better standards for governance. Evidence of this "improving standards" or "postmaterial values" effect could be detected if we observe that economically satisfied corruption perceivers score higher on indicators of political accountability or postmaterial values than similarly economically satisfied "low or no" corruption perceivers (or less satisfied corruption perceivers). If this effect is absent, however, then we can most likely rule out the possibility that the steeper presidential penalty in this particular substratum of the population has to do with postmaterial values or improving standards for governance.

Improving standards or postmaterial values can be proxied by a number of questions in the LITS survey. Specifically, I tested 1) whether highly satisfied corruption perceivers are more likely to agree with the statement "As citizens, we should be more active in questioning the actions of our authorities" (as opposed to "in our country today, we should show more respect for our authorities"); 2) whether they are more likely to agree with the statement "democracy is preferable to any other form of political system" (as opposed to the statements "under some circumstances, an authoritarian government may be preferable to a democratic one" and "for people like me, it does not matter whether a government is democratic or authoritarian"); 3) whether they are more likely to believe that "elections are necessary" for leaders of local and regional administrations (as opposed to believing that "leaders should be appointed" or "it does not matter"); and 4) whether they are more likely to believe that certain behaviors like "a public official asking for a favor or gift in return of services" or "buying a university degree that one has not earned" are seriously wrong (on a scale from "not wrong at all" to "seriously wrong").

The results indicate that the higher presidential penalty among highly satisfied corruption perceivers does not stem from an underlying postmaterial attitudinal basis (See Appendices B12-B15 for descriptive statistics, results and descriptions of the probit models). It appears that people who are increasingly satisfied with the economy (both at the national and household levels) are in fact marginally *less* likely to have an absolute or relative belief that citizens should be more active in questioning the actions of their authorities than their less economically satisfied counterparts. At the same time, however, corruption perceivers do not differ in critical attitudes toward authority from non-corruption perceivers at any level of economic satisfaction. The lack of an interaction suggests that economic satisfaction does *not* influence corruption perceivers' presidential attitudes through a change in their underlying orientations toward authority. A "postmaterial effect" of this kind might still be in play over time, but we do not observe differences in such authority orientations among different economically satisfied strata in the cross-sectional sample.

The same null interaction effect occurs among the other "postmaterial" proxies. Economically satisfied corruption perceivers (at both national and household levels) are not more likely than similarly satisfied 'low or no' corruption perceivers to believe in democracy, desire elections for local or regional governments or believe that certain traditionally "corrupt" practices are seriously wrong. Nor is there a difference on these attitudinal indicators between high corruption perceivers at different levels of economic satisfaction.

Granted the lack of an interaction in these sensitivity checks, we can tentatively rule out the possibility that higher standards for governance influence corruption perceivers' presidential approval patterns. If the economically moderated correlation between corruption and presidential trust truly arises from corruption resentment, it appears entirely plausible that people simply find the necessity to pay bribes at schools, hospitals and other public institutions more jarring and

unacceptable in good economic times without adopting particularly sophisticated liberal notions of political accountability. Next to new cosmopolitan boulevards and cafes, broken hospitals and schools can appear discordant with the improving living conditions, and may inspire blame attribution toward the presidency as a matter of pure frustration with festering institutional dysfunction. Testing this feeling of enhanced exploitation is difficult with the current survey data. We could posit that highly economically satisfied corruption perceivers are more likely to be upset with the institutions that force them to pay bribes than their less economically satisfied counterparts. One question in the LITS survey inquires precisely about people's satisfaction with the "quality and the efficiency of the service or interaction" which they previously rated on corruption. Unfortunately, such questions are of little use because of their high rates of nonresponse. Seven out of eight of these survey questions have nonresponse rates above 74 percent, and only one (a question concerning satisfaction with medical treatment at state hospitals) has a lower nonresponse rate (31.32 percent), which still makes for an unreliable indicator in the present attempt at a sensitivity check. Testing the perceived enhanced exploitation of corruption could benefit from field or lab experimental applications in which corruption perceivers are granted the opportunity to express or choose narratives describing the political targets and reasons for their blame attribution in good economic times. Such a design would produce results with limited external validity, but would certainly aid in teasing out causal mechanisms that we cannot identify with the present research design.

Although the story of the precise mechanism remains underexplored, several findings are clear from the above analysis. There is no evidence in either Latin America or the post-Soviet space suggesting that people trade off concerns about petty corruption for economic gains. Corruption that poses a direct cost to people remains a significant corrosive influence on their

presidential approval. Although presidents in Latin America might successfully get away with transgressions in good times, they do not manage to fool people about the quality of public institutions in their countries. There is also a marginally higher presidential penalty among economically satisfied petty corruption perceivers in both empirical contexts, although it appears that this penalty is not the result of rising standards for governance.

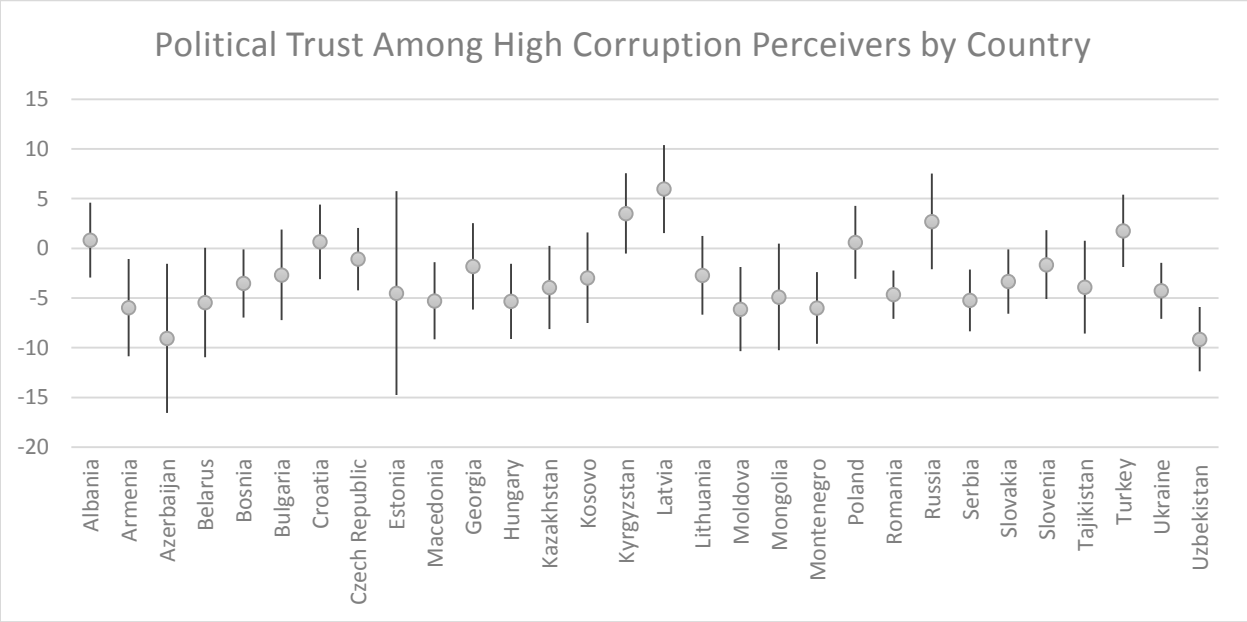
Implications for Authoritarian Political Legitimacy

My findings also have implications for the study of authoritarian political legitimacy. In this study I included parts of Central Asia and the Southern Caucasus which appear to be driving up the correlation between corruption and political trust across countries in the former Soviet space. Inside those countries, however, the story is different. It bears noting that while autocracies appear better than democracies at eliciting trust, this is not a uniformly *uncritical* form of trust. Political institutions appear to be seen as more accountable for petty corruption in autocracies than democracies. These results would be implausible if citizens of autocracies overwhelmingly inflated their trust responses out of fear, if they were governed by a hard-to-measure collectivistic culture or if they lacked notions of accountability. Critical attitudes toward authoritarian political systems among high corruption perceivers suggest precisely that people respond rationally to institutional performance. In fact, given the possibility of preference falsification, the interaction estimate in this regression is a conservative one, and the real impact of corruption perceptions on trust in autocracies is likely steeper.

To diagnose the interaction effect more carefully, I plotted the point estimates of the political trust scores among high corruption perceivers by country, maintaining the standard sociodemographic controls (Figure 4.1). These estimates represent the extent to which political

trust among high corruption perceivers differs from 'low or no' corruption perceivers on the 1-100 trust scale. In over half of the sampled countries, there is no significant difference between the two groups. The autocratic effect we observe in the pooled regression is driven primarily by the low point estimates in Uzbekistan and Azerbaijan (where the estimated political trust scores are close to 10 points lower among high corruption perceivers than 'low or no' perceivers—this is about twice the effect of the lowest point estimates in other countries.). The corruption effect is actually absent in autocracies like Kazakhstan and borderline insignificant in Belarus. At the same time, Armenia, Macedonia, Hungary, Moldova, Montenegro, Serbia and Ukraine *do* maintain significant corruption effects on trust, even if not quite as steep as those in Uzbekistan and Azerbaijan. There remains meaningful variation in corruption responsiveness within the roughly cut regime spectrum. Certainly, not all autocracies in general, nor Central Asian autocracies in particular, are the same.

6 **Figure 4.1 Political Trust among High Corruption Perceivers by Country**



Note: The graph describes point estimates of political trust among those regularly exposed to petty corruption (relative to those rarely or not exposed) in 30 separate OLS country regressions maintaining sociodemographic controls. Bars

are 95 percent confidence intervals. Example: high corruption perceivers in Azerbaijan trust their political institutions approximately 10 points less than 'low or no' corruption perceivers in Azerbaijan.

Granted the relatively steep corruption effect in two of the most authoritarian regimes in the sample, my findings are *somewhat* consistent with Huhe and Tang's (2016) research on political trust in East Asia, which suggests that citizens of autocracies are more sensitive to political outcomes than citizens of democracies. The authors argue that in democracies "with institutionalized competition, effective electoral procedures, and resultant office alteration, the incumbent governments or particular politicians, and not the overall political system, are likely to be held accountable for economic performance" (ibid., 5). By contrast, in countries like China and Vietnam where power is more concentrated in a few hands, political institutions as a whole take more of the blame during economic downturns.

It was not a far step to test Huhe and Tang's hypothesis with more clarity. If citizens of autocracies hold political institutions more accountable for corruption, they should also hold them more accountable for economic performance. I found preliminary evidence to support this theory, again with some nuance (Table 4.6). Citizens of democracies who are maximally satisfied with their financial situation (scoring a 5 out of 5 on satisfaction) trust political institutions 5.6 points (out of 100) less than those who are maximally satisfied with their financial situation in autocracies. Those who score a 4 out of 5 on financial satisfaction in democracies trust political institutions 7.5 points less than those who score a 4 out of 5 on financial satisfaction in autocracies. The effect is more striking for sociotropic evaluations of the economy. Citizens of democracies who are maximally satisfied with the national economy trust political institutions 12.7 points less than citizens of autocracies who are maximally satisfied with the national economy. Citizens of democracies who score a 4 out of 5 on satisfaction with the national economy trust political

institutions 11.8 points less than citizens of autocracies who score a 4 out of 5 on satisfaction with the national economy. In fact, *every* perceived marginal improvement in the country's economy significantly boosts political trust at least 7 points more in autocracies than democracies. The effect does not appear for consumption quartiles or the ladder question. Interestingly, there is some evidence that citizens of democracies on the highest part of the ladder (steps 8-10) trust political institutions about 6 points *more* than citizens of autocracies on the highest part on the ladder. This positive effect holds only for the proportion of people on top of the ladder, however; on all lower steps of the ladder, citizens of democracies are either no different from or less trusting than citizens of autocracies.

13 **Table 4.4 Regime moderation of economic influence on political trust**

(DV= political trust scaled 1-100, fixed effects OLS, 30 countries)

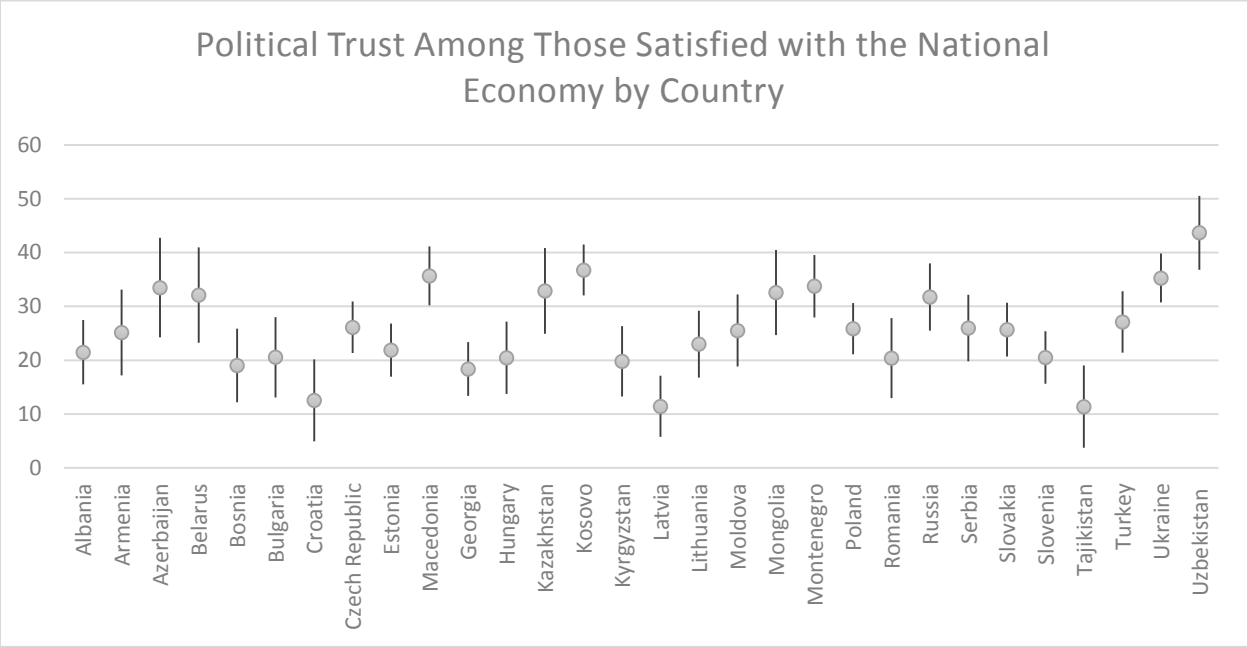
	Satisfied with financial situation	Satisfied with national economy	Consumption Quartiles	Ladder Question
Economic Context (see column heading)				
2	11.739*** (1.539)	17.520*** (1.814)	0.439 (1.204)	8.385*** (1.409)
3	18.112*** (1.556)	28.424*** (1.766)	-0.131 (1.213)	11.887*** (1.449)
4	26.314*** (1.517)	37.708*** (1.729)	-4.066*** (1.229)	12.622*** (1.547)
5	26.825*** (2.209)	37.859*** (2.225)	N/A	10.304*** (2.328)
Ref= Autocracy				
Anocracy	-6.600*** (1.940)	2.332 (2.124)	-10.220*** (1.551)	-8.713*** (1.964)
Democracy	-35.399*** (1710)	-28.219*** (1.920)	-41.709*** (1.370)	-41.733*** (1.689)
Regime x Econ Interactions	Sat. with financial situation x Regime	Sat. with national economy x Regime	Consumption quartiles x Regime	Ladder question x Regime
2 x Anoc	**-2.275 (1.872)**	**-9.406*** (2.038)**	**-3.159* (1.629)**	**-2.665 (1.823)**
2 x Democ	**-3.759** (1.611)**	**-7.065*** (1.858)**	**-1.410 (1.300)**	**-3.921*** (1.497)**
3 x Anoc	**-3.029 (1.902)**	**-10.826*** (2.049)**	**-0.472 (1.632)**	**-2.413 (1.876)**
3 x Democ	**-4.759*** (1.629)**	**-8.817*** (1.826)**	**-1.033 (1.305)**	**-3.689** (1.545)**
4 x Anoc	**-4.568** (1.894)**	**-13.070*** (2.085)**	**0.936 (1.642)**	**-1.480 (2.065)**
4 x Democ	**-7.570*** (1.604)**	**-11.840*** (1.822)**	**1.691 (1.315)**	**-0.059 (1.654)**
5 x Anoc	**-4.653 (3.063)**	**-10.865*** (3.153)**	**N/A**	**4.585 (3.325)**

5 x Democ	**-5.619** (2.404)**	**-12.619*** (2.471)**	**N/A**	**6.168** (2.523)**
Middle Ed	0.213 (0.359)	1.068*** (0.348)	1.329*** (0.405)	0.245 (0.369)
Upper Ed	-0.256 (0.378)	1.193*** (0.364)	1.865*** (0.425)	0.035 (0.388)
R^2	0.321	0.362	0.269	0.284
N	28423	28294	24516	28671

Note: Key interaction effects are in bold. Controlled for female, age, age squared, household size, urban and capital dwellers (see Appendix B16 for full table). Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01

Once again, I diagnosed this interaction more carefully by plotting the country regressions side by side (Figure 4.2). Responses of 5/5 on satisfaction with the national economy are scarcer than other options and yield higher standard errors, so I plotted political trust among respondents who scored a 4/5 on satisfaction with the national economy (relative to the least satisfied) to enhance accuracy in the cross-national assessment. Uzbekistan again stands out with the highest point estimate. Azerbaijan, Belarus and Kazakhstan also exhibit notably higher estimates than other countries, but these are not much different from Macedonia, Kosovo, Mongolia, Montenegro, Russia and Ukraine, where people also trust political institutions by about 30 points more than the least economically satisfied strata in their respective countries. On the whole, however, the averaged estimates we get from the pooled regression summarize this variation accurately: citizens of most countries in Eastern and Central Europe do not reward political institutions for economic satisfaction as much as those living in autocracies further east.

Figure 4.2 Political Trust Among Those Satisfied with the National Economy by Country



Note: The graph describes point estimates of political trust among those who score a 4/5 on satisfaction with the national economy (relative to the least satisfied) in 30 separate OLS country regressions maintaining sociodemographic controls. Bars are 95 percent confidence intervals. Example: Citizens of Azerbaijan who score a 4/5 on satisfaction with the national economy trust political institutions by about 34 points more than those who score a 1/5.

This additional piece of analysis can help explain why political institutions bear the brunt of petty corruption more heavily in authoritarian states in the former Soviet space. Since incumbents are not renewable and political institutions do not function independently of the incumbent, people perceive that their wellbeing overwhelmingly depends on the behavior of those in power. Although political institutions in autocracies take disproportionate credit for economic outcomes, they also take disproportionate blame for petty corruption. We tentatively observe the same elevated sensitivity to political outcomes in authoritarian polities in this sample as Huhe and Tang observe in their East Asian counterparts. At the same time, there are interesting differences between autocracies in the region which should preclude any hard convictions about a logic of

public opinion in authoritarian regimes, and which should inspire crisper data collection and future substantive research about the nature of political trust and accountability in closed societies.

While I found that high corruption perceivers trust the presidency even less than political institutions in a smaller subset of presidential systems, I was unable to determine the particular way in which regime types influence the effect of corruption perceptions on presidential trust because the smaller sample had too little variation in regime types to permit a reliable comparison. The weaker correlation between corruption perceptions and institutional trust suggests that trust in political institutions represents a diffuse form of support (using Easton's distinction) whereas trust in the presidency implies a more immediate target of accountability. Nevertheless, even if the presidency is held *especially* accountable by well-off corruption perceivers, citizens of autocracies still hold *entire* political systems accountable for political outcomes more than citizens of democracies. This is consistent with my findings in Chapter 2, which show that citizens of autocracies do not distinguish as much between branches and levels of government.

These patterns offer somewhat of a cautionary tale for autocracies hoping to sustain legitimacy through occasional economic stimulus without reforming corrupt institutions. In line with Huhe and Tang's arguments, "authoritarian regimes, although they may enjoy all the benefits entailed by economic growth, are also likely to take full responsibility for any economic downturns" (ibid., 5). My findings show that not only is this likely the case, but economic growth can be a double edged sword for authoritarian legitimacy: people who perceive high levels of petty corruption potentially hold the presidency even *more* accountable as they become more pleased with their financial situation and the national economy. Economic improvement at the individual level is more likely to spur critical rather than complacent political attitudes, but the precise reasons for this deserve future study. On the one hand, as English philosopher Thomas Hodgskin lectured

in 1843, perhaps "people want food, and wanting food, want clothing, want cleanliness, want comfortable habitations, want decency, want kindliness, and want morality…" (Hodgskin 1843, 15). On the other hand, sensitivity checks suggest that morality has nothing to do with this steeper decline in presidential trust.

The lack of evidence for a corruption tradeoff effect in good times suggests that there is more to performance legitimacy than economic development. Regardless of how well off people believe they are, petty corruption consistently erodes their political trust, particularly in autocracies like Azerbaijan and Uzbekistan. Under the bubbling surface of high growth and propaganda, people are not happy with exploitative and corrupt public institutions. Once the fuel runs out and authoritarian elites are no longer able to supply the economic performance they have ridden in the post-Soviet period, their political trust levels are likely to suffer faster downward swings than what we would expect of democracies. When those economic downturns occur, political legitimacy in these regimes will likely depend more exclusively on the strength of their coercive apparatuses.

5 Chapter 5. Economic Growth and Anti-Putinism

5.1 Introduction[17]

In the previous chapters I have focused mainly on the influence of economic attitudes on how corruption victims evaluate their political institutions. While political and economic attitudes are intimately causally interconnected, the influence of objective improvements in material wellbeing on political attitudes is more tenuous. When does real economic growth reinforce or undermine the incumbent's position in politically closed societies? I will consider this question with a case study in this chapter.

Russia's GDP per capita adjusted for purchasing power parity rose from $6825 in 2000 to $24,000 in 2011, a 252 percent increase comparable to China's rise from $2915 to $10,270 in the same period. The percentage of Russia's population living below the national poverty line halved between 2002 and 2011. Life expectancy at birth rose from 65.3 years in 2000 to 69.7 years in 2011. Annual growth rates between 2000 and the financial crisis of 2008 were at least triple the growth rates of any year after the fall of the Soviet Union.[18] Towards the end of Russia's extraordinary decade of growth, the largest protests in post-Soviet history broke out against then-Prime Minister Putin in December 2011. Although growth has often been considered the main source of Putin's tremendous popularity (Treisman 2011) much like Chinese growth has been credited with bolstering the Chinese Communist Party (Wong et al. 2011, Yang and Tang, 2010), many speculated by the end of the decade that growth had produced an unintended threat to the Russian incumbent by facilitating the adoption of post-material values (Dmitriev and Treisman

[17] Supplementary materials for this chapter are available in Appendix C.
[18] Data obtained from World Development Indicators at the World Bank http://data.worldbank.org/

2012). No longer worried about survival, Russian citizens seemingly became more concerned about political rights and good governance.

The literatures on social movements and political trust make empirically supported but not fully compatible claims about the long-term political consequences of economic growth. On the one hand, economic growth spurts result in the gradual development of post-material values which can lead to protests and demands for democracy (Inglehart and Welzel 2005). On the other hand, a long period of economic growth has contributed to Putin's overwhelming approval ratings, which, despite occasional dips, remained at 63 percent of the population even when protests initially broke out in December 2011. These ratings would not dip below 61 percent as of the time of this writing—and have fluctuated between 80 and 88 percent since the annexation of Crimea.[19] Although the 2011-12 outbreak of protests seems compatible with theories of post-materialism, Putin's high-to-colossal approval ratings speak to the strong trust-promoting effects of positive economic attitudes found in the political trust literature. It would appear that it is now simultaneously easier and more difficult for Putin and his inner circle to buy off Russian society in the interests of political survival. Which of these scenarios is closer to the truth?

We do not yet have a clear sense of when economic development is a boon or a burden for incumbent legitimacy. One possibility in the Russian case is that economic growth is simultaneously promoting and deflating presidential approval depending on variation in political openness and historical income conditions across Russia's 84 regions. That is, although the country as whole has undergone an impressive decade-long developmental climb since the late 1990s (coinciding almost perfectly with Putin's tenure), economic growth among initially wealthy

[19] See Levada Center polls at http://www.levada.ru/en/about-us/

regions might not be driving up presidential approval as much as economic growth among initially poor regions. At the same time, regions with higher political competition and ties to the west might prove more impervious to the trust-promoting effects of economic growth. Even though the uneven distribution of economic development and political openness across Russia's vast territory has been widely documented (Lankina 2015; Petrov 2005; Popov 2001), the literature on political trust and presidential approval in Russia has thus far eluded a proper contextual analysis of the country's conflicting approval patterns.

Another part of the reason we are relatively short on nuanced theory about the attitudinal implications of growth is that growth simply has not been a big part of the equation in political trust or incumbent approval research. The social movement literature tends to measure the effects of current socioeconomic conditions on protest activity while the political trust literature relies on the effects of people's perceptions of economic performance on public opinion. Although contributors to both discussions often speculate about the consequences of *growth,* commonly used predictors like regional GDP or attitudes toward the economy are static and subjective. My goal in this paper is to take what is best from these literatures to capture the effects of *growth* on presidential approval while accounting for its contextual interactions across the country.

Studying this link has important implications for our understanding of authoritarian survival around the world. Economic growth tends to boost incumbent survival (Bueno de Mesquita and Smith 2010; Marinov 2005), allowing leaders to maintain a favorable status quo, build patronage networks and quash opposition. Across autocracies, government elites tend to defect less from hegemonic parties during periods of growth (Reuter and Gandhi 2011; Magaloni 2008, 15). For Treisman (2009), this growth-induced legitimacy is a key but underappreciated element of the political economy of post-socialist transition. President Yeltsin faced oppositional

coalitions in the Duma during Russia's massive economic contraction after the fall of the Soviet Union. Rapid development at the start of Putin's presidency in turn removed hindrances to his ability to create a legislative coalition and steadily suffocate the opposition. Elites apply oppositional pressure on the incumbent when they have the backing of the mass public, making public approval ratings of the incumbent a key part of their strategy building (Treisman 2009; Hale 2005, 2010). In Treisman's narrative, mass satisfaction with the economy in the early 2000s inspired representative bodies to let their guards down as Putin carried out his destructive agenda. At least in the short term, the broader literature suggests that periods of growth undermine systems of checks and balances in favor of authoritarian consolidation.

In the long term, however, autocratic states with high rates of growth tend to democratize (Geddes 1999). It has long been argued that only wealthy societies can maintain the ability to participate in politics without succumbing to demagoguery or tyranny (Lipset 1959, 75). As noted above, in modernizing autocracies an aptitude for democratic participation can arise from the triumph of post-material values over survival priorities. Transitions to democracy, however, are not likely to occur until incumbent dictators exit office (Treisman 2014a). It remains to be seen whether Russia will follow the path of Spain, which quickly democratized in 1975 after experiencing quadrupling GDP growth under four decades of rule by Franco. Rapid growth can set into motion a runaway train resistant to any attempts of an incumbent dictator to stop a full-on derailment.

Although an exact prediction of Russia's trajectory is impossible, it *is* possible to reduce the level of abstraction in such a discussion by considering the dynamics of growth within the country. Hybrid regimes like Russia which combine some electoral contestation with central control by a political machine are nevertheless vulnerable to shifts in political trust and street

protests (Robertson 2013). Given the diversity of economic development and political openness across Russia's vast territory, it is possible to pinpoint the government's local vulnerabilities to anti-incumbent challenges. While such a case-oriented approach sheds light on Russia's political dynamics, it also helps us theorize about the conditions under which economic growth threatens or promotes incumbent survival in hybrid political regimes.

5.2 Empirical Considerations

Economic performance can be seen as a proxy for the competence of the country's leadership, especially when the electorate lacks other information or criteria against which to assess its performance (Burke 2012, 2). Positive economic evaluations usually have significantly more explanatory power over political trust and presidential approval than other performance-based variables in Russia (McAllister and White 2007; Rose, Munro and Mishler 2004; Mishler and Willerton 2003; Treisman 2011), China (Wong et al. 2011, Yang and Tang, 2010), Latin America (Zechmeister and Zizumbo-Colunga 2013) and Africa (Lavallée et al. 2008). Despite the persistently strong influence of economic sentiment, economic performance is not a good predictor of cross-national differences in political trust (Wong et al. 2011, 273; Van der Meer and Hakhverdian 2016, 4).

Evidence of the influence of objective economic performance on political attitudes is mixed. Clarke et al. (1993) show that growth in unemployment and inflation significantly depressed support for governing parties in eight Western European countries between 1976-1986 while Van Erkel and Van der Meer (2016) somewhat contradict this finding by arguing that unemployment reduces political trust while economic growth and inflation enhances it over time in fifteen EU countries between 1999-2011. In several cross-sectional studies, Van der Meer and Hakhverdian (2016) find that macroeconomic indicators have no effect on confidence in

government across 48 European countries, confirming the null effect found in Hakhverdian and Mayne (2012) between country-level GDP and institutional trust.

Why are correlations of subjective attitudes so strong and objective indicators so tenuous? What do subjective attitudes capture, if not objective economic conditions? For one, regressions of attitudes on attitudes are subject to spurious correlations and reverse causation. Although economic sentiment enhances political trust, people are just as likely to rate the economy more favorably because they approve of current political institutions—not because they are actually better off. Economic perceptions are also functions of individual characteristics, ideological proclivities and sources of exposure to information about politics—not all of which are immediately measurable or controllable. They also capture different benchmarks against which people judge economic performance, whether they are cultural, regional or longitudinal. Given globalization and the spread of information, people might be ranking their wellbeing or national economic performance against how they perceive other countries are doing (Deaton 2008, 70; Van der Meer and Hakhverdian 2016, 6).

In the Russian case, the limited evidence we have is unclear about the effect of objective economic conditions on political support. Using Levada Center opinion polls, Treisman (2009, 10) finds that although Russians' evaluations of the economy were higher than warranted in the lead up to elections in 1996 and 2004, on the whole they reflected variation in objective indicators like real wages, pensions, wave arrears, unemployment and job openings. Colton and Hale (2009, 493) similarly argue that Putin's approval throughout the 2000s was driven more by positive evaluations of the economy than any policy initiative in the Putin administration. At the same time, however, they use survey data to argue that the bulk of Putin voters had experienced no increase to their personal wellbeing over the course of his presidency, concluding that sociotropic economic

evaluations reflect state-media exposure and positive expectations of the future rather than objective improvements in material welfare. Even this interpretation is based on self-reported income rather than an objective evaluation of income or its growth. It is important to use more objective measures to avoid conflating the effects of growth on political trust with the effects of the government's attempts to manipulate public opinion—or, indeed, with any number of influences on attitudes which do not relate to objective material wellbeing.

The effect of growth itself remains unclear in the broader literature. Popularly used measures of income *levels* can tell us how much people trust political institutions based on their income, but they do not tell us what happens to attitudes when those incomes change. In a recent breakthrough on the effects of growth on political attitudes, Guriev and Treisman (2016, 22) use the Gallup World Poll to show that annual GDP growth rates are straightforwardly connected to government approval between 2005-2014 in the UK, Brazil, Turkey, Zimbabwe and Venezuela, but, interestingly enough, they are not straightforwardly connected in Russia. The authors explain this away by attributing temporary dips and spikes to a presidential election, war in Georgia and the financial crisis. Despite event-based fluctuations, however, the *long*-term relationship between growth and government approval remains tenuous in a country where growth is generally believed to play a stronger role in the formation of political attitudes than any other variable (as argued by Treisman himself on many occasions). In a follow up to the article, Guriev (2016) argues that the social contract between the Russian government and its citizens after the growth decade may be moving away from a promise of economic betterment to a promise of reclaimed geopolitical status. Even if that is the case, the lack of a clear relationship between growth and approval between 2005-2010 (see their graph on page 22), before the change in the social contract, is puzzling. If government approval is so sensitive to temporary events like elections during a decade of intense

growth, it would appear that the growth rate is not, after all, so crucial to incumbent legitimacy. Or are we missing other factors?

5.3 Contextual Effects

The picture could be incomplete because we are not yet properly diagnosing contextual growth effects on incumbent approval within the country. Inferring things about individuals in a country based on cross-national trends can, of course, put us at risk of committing an ecological fallacy. Yet local context can be quite important to incumbent support across internally heterogeneous societies and rapidly growing non-democracies where citizens are more sensitive to economic performance than their counterparts in democracies (Huhe and Tang 2016; Tang et al. 2015). Taking into account large imbalances in development across China, Hutchison and Xu (2016) argue that regional economic performance significantly enhances political trust and (so far) offsets the negative effects of regional inequality and openness to global markets. Interestingly, Su et al. (2016) show that provincial level GDP in China *lowers* trust in the central government. Neither study considers regional differences in economic *growth*, but it is reasonable to suppose that uneven distributions of growth and institutional openness across both Russian and Chinese subnational regions produce out of sync incumbent approval ratings which give rise to competing theories about the trust-promoting and post-material effects of growth. With regards to Russia, both effects are probably in play depending on when and where we look.

Russia is a particularly fruitful case study because of its immense internal variation in economic performance. As of 2009, GDP per capita adjusted for purchasing power parity in the top three performing regions was equivalent to that of Norway, Hong Kong and the Netherlands, while the bottom three regions were equivalent to Turkmenistan, Bhutan and Iraq. The unevenness of economic development across Russia is determined both by varying endowments of natural

resources and Soviet practices of concentrating the production of certain goods and services in specific locations (Van Selm 1998, 605). Regional success in the post-socialist transition heavily depended on regional specialization in competitive industries like fuel and energy production as opposed to non-competitive ones like machinery building and agriculture (Popov 2001, 877). Even in 2014 average monthly income per capita was 2.7 times higher in oil rich Tyumen than in the resource poor, non-industrial Republic of Tyva. Yet some inherited differences have seen big transformations in the course of the post-socialist transition. While the Sakha region outperformed the republic of Dagestan 5.4 times in average monthly income per capita in 1995, it reduced its lead to 1.5 times by 2014. Regions across Russia have had staggered growth spurts over the previous two decades.[20]

Geographer Natalia Zubarevich (2012) describes four "Russias" which have spun out of the transition: 1) the land of large post-industrial cities with over 250,000 inhabitants, 2) the land of blue collar workers in smaller company towns dependent on non-competitive industries, 3) the land of rural and semi-urban populations and 4) the land of non-industrial undeveloped areas capturing the northern Caucasus. While Putin has largely been able to buy off the support of populations in categories 2-4 with direct welfare transfers from the federal budget, the growth of medium-to-large cities in the first category (currently home to a little under half of the Russian population) has given rise to a mobile, educated, internet-savvy middle class that is not significantly tied to the state, one location or one job. It is "an electorate that won't stay zombies for long," she argues, suggesting that long-term demographic shifts may give rise to expectations for reform beyond the ability of the government to control (ibid.).

[20] Data obtained from the Russian Federal State Statistics Service (Rosstat) www.gks.ru

Trends in protest activity across Russia provide some insight into this long-term shift. The number of protests since the 1990s has steadily increased and become more politicized (Lankina 2015, 39). Robertson (2013, 19) shows that protest demands in the 2000s have shifted from concerns about the unpaid wages and poverty of the transition period to concerns about political and civil rights, the environment and urban planning. He speculates that these qualitative changes "are early warning signs that the implicit exchange of economic growth for political stability of the early Putin years may be coming undone" (ibid.). While the 2011-2012 anti-Putin protest wave drew the largest crowds in Moscow and St Petersburg, most protests took place outside the two capitals in regions that are comparably wealthier, more politically open and independent of handouts from the federal center (Lankina 2015, 34). Protest levels did not correlate with regional GDP levels, making it somewhat puzzling that citizens with more socioeconomic grievances did not choose to air them publicly (ibid., 31-32). Lankina's observation is ultimately difficult to interpret because it is based on the total number of protests per region without accounting for the size of the regional populations.

Nevertheless, Russia's poorest regions—particularly those in the northern Caucasus—also tend to host the most repressive political systems in the country, which reduces the amount of protest mobilization in the region that gets recorded by journalists and social scientists. Recent work suggests that Russia's regime hybridity is a spatial phenomenon (Lankina 2015; Lankina and Voznaya 2015; Libman 2012). That is, elements of electoral competition and political liberty are selectively incorporated into a vertical power structure at both national and regional levels. After the fall of the Soviet Union, a weak federal state meant that regions could develop diverse local political systems. Since the rise of Putin and his abolition of the popular election of regional governors in 2004, pockets of regional autonomy have nevertheless persisted. Some regions have

managed to retain the popular election of mayors who can make alliances with local civil society and enhance media freedom if it helps them counteract regional governors (Lankina 2015, 28). Despite the efforts of the federal center to undermine local initiatives, incentive structures within municipal politics generate significant, lasting differences in regional voting patterns, elite turnover and civil society activity (ibid.). Regional authoritarianism also tends to be reinforced through trade with former Soviet Union countries but mitigated by inflows of EU aid to civil society partners (Lankina et al. 2016). If elites truly do follow public opinion as Treisman and Hale suggest, it is possible that regions which have benefited most from economic growth and relatively open political structures are ones where local elites can, given a political or economic crisis, take advantage of low incumbent approval ratings to challenge the center.

5.4 Hypotheses

Judging by protest statistics alone, however, it would be premature to conclude that incumbent disapproval comes from the fastest growing regions. Internal variation in Russian economic development, political openness and anti-regime mobilization suggests that the influence of growth on incumbent support is likely subject to a set of complex interactions which are contextual, rather than individual in nature. Several potentially important interactions come to mind. I begin with a simple unidirectional hypothesis (*H0*) that economic growth has a positive effect on approval, moving toward more nuanced hypotheses below.

H1. The effect of economic growth on incumbent approval depends on the length of time regions have been growing.

We know that economic growth tends to enhance the survival of incumbent dictators in the short term, and any translation of income growth into political liberalization often takes 10-20

years or the removal of the incumbent from office (Treisman 2014a). At the level of attitudes, Inglehart's (1990) theory about the post-material effects of growth is based on decade-long trends of declining trust in government among advanced industrialized democracies in the post-war era. The adoption of worldviews which emphasize self-actualization over survival develops over the course of one or more generations and usually cannot be triggered at the onset of growth. With regards to Russia, major protests and demands for better governance developed only after the country's decade of hyper growth had ended. Given the uneven level of development across the country, I expect that regions which have experienced sustained growth will have lower incumbent approval ratings toward the end of the decade than regions which have experienced more recent growth spurts. The proposition here is that longer-term growth leads to more critical political attitudes while recent growth enhances incumbent support.

H2.The effect of economic growth on incumbent approval depends on initial regional economic performance.

Regions which are better off to begin with might experience diminishing returns from economic growth. After attaining a certain level of development, more growth will likely do increasingly less to improve one's view of political authorities. As suggested in the social movement literature, citizens in historically more developed and cosmopolitan regions are more active in pursuit of higher order demands like justice and better governance. At the same time, citizens in initially poor regions which are experiencing "catch up" growth might not yet be ready to exchange additional material gains for better governance. The proposition here is that growth will not boost incumbent support as much in initially wealthy regions as it will in initially poor regions.

H3. The effect of economic growth on incumbent approval depends on regional political openness.

Unlike their authoritarian counterparts, politically open regions are more likely to be exposed to the west, have more independent media exposure, host more civil society organizations and have histories of protest activity. I expect that the diffusion of democratic values proceeding from such factors will lower incumbent support, and that this negative effect will mitigate the positive influence of economic growth in these regions. The proposition here is that economic growth will not enhance incumbent support as much in politically open regions as it will in less open regions.

5.5 Analytical Strategy

To test these propositions, I was able to obtain incumbent approval data in February 2011 from the Public Opinion Foundation (*Fond Obschestvennogo Mneniya*, or FOM), regional economic development data from the Russian State Statistics Agency (*Rosstat*) and regional democracy data from the Independent Institute of Social Policy (IISP) in Moscow. Unfortunately I was unable to get access to longitudinal incumbent approval data from FOM due to restrictive subscription costs. This somewhat limits, but does not totally preclude an analysis of a growth effect on incumbent approval.

I chose to look at approval data from February 2011 for several reasons. This time period follows Russia's decade of growth, allowing us to measure long-term regional growth effects on incumbent approval. At the same time, it was a relatively "normal" time for incumbent approval in the sense that political attitudes in February would not have been temporary shifted by sudden shocks or events; it was not a presidential election year and the economy was largely recovered

from the 2008 financial crisis (which depressed approval ratings at the time, and unevenly so across regions). February 2011 also precedes Putin's announcement of returning to the Presidency in September and fraudulent parliamentary elections in December which triggered mass protest activity that would last through the 2012 presidential election season. These events would have been unforeseen in February 2011; indeed, the public response to such triggers would likely not have been so intense were there indications of major political breaches earlier in the year. Putin's approval rating decreased fairly dramatically over the year, from 79 percent in December 2010 to 63 percent in December 2011. Yet if we examine the data from the Levada Center, approval ratings remained relatively stable in the upper 60s throughout 2011 until a more concerted drop after the onset of protests in December. Approval ratings in February 2011 still caught the upper end of that approval trend at 72 percent. If economic growth does affect incumbent approval as I think it does, it is fair to suggest that some amount of incumbent disapproval arising from long-term economic shifts will have predisposed parts of the public to protest at the end of the year. These anti-incumbent predispositions should be detectable in February 2011, before they were triggered into action.

FOM conducts quarterly nationwide surveys, randomly selecting household samples within districts to match their population characteristics (see Ananyev and Guriev 2016, 13-14). The February 2011 data consist of 54,388 households, offering significantly more coverage than any other publicly available dataset that I know of (such as the LITS, local opinion barometers or the World Values Survey, each offering approximately 1000 households). Since the large N of the FOM survey allows for more observations within regions (between 500 and 800 per region) a contextual analysis of regional incumbent support becomes considerably more reliable.

5.5.1 Dependent Variable

By "incumbent approval" I mean approval of Vladimir Putin. Even though he was Prime Minister in February 2011, he had served as the country's President since the onset of the growth period and would return to the post a year later. Putin would have been held responsible for economic performance from 2000 to the current day. Although Dmitry Medvedev was president in February 2011, Treisman (2014b, 8) shows that approval ratings for Putin and Medvedev throughout their tenures were almost perfectly correlated, suggesting that the latter was under the control of the former, and that the public did not distinguish between the two figures.

To measure incumbent approval, the FOM survey question reads, "Is the head of the government Vladimir Putin performing well or poorly at his job? Has his work in recent times been improving, deteriorating or has it remained unchanged?" Respondents have six response options: "1) He performs well and is improving; 2) he performs well and hasn't changed; 3) he performs well, but is deteriorating; 4) he performs poorly, but is improving; 5) he performs poorly and hasn't changed; 6) he performs poorly and is deteriorating." To capture approval, I will create a dummy variable where '1' indicates respondents who believe Putin is performing well and is either improving or staying the same (items 1 or 2) and '0' indicates the rest. It might be argued that such a question is problematic because it directs respondents' attention to Putin's recent performance and may therefore cause us to underestimate the long-term regional growth effect on incumbent approval. My hunch is that this may not matter because 1) the first part of the question focuses on a generic performance evaluation regardless of time span, and 2) long-term economic shifts are structural factors which will likely produce different associations of Putin in respondents' minds regardless of the question's reference to changes in his performance in "recent" times. Nevertheless, we could detect such an underestimation effect by conducting robustness checks to

alternative measures of incumbent support available in the survey, including evaluations of President Medvedev, the government in general and whether respondents would vote for Putin's party, United Russia, should elections be held the following Sunday.[21] If the different kinds of questions lead to very different outcomes we might have evidence that such differences can be attributed to features of the approval question.

It must be noted that approval of United Russia and/or Putin himself may not symbolize active enthusiasm as much as resigned acceptance, particularly because citizens may feel that there is no real alternative to the current power structure. Nevertheless, respondents have neutral and non-response options which give them a chance to mark something other than the incumbents even if they feel there is no real alternative. Approval ratings in a hybrid regime might also reflect preference falsification due to repression rather than real attitudes. On repeated occasions, however, this argument has not held water in the Russian context (Frye et al. 2016; Guriev and Treisman 2016; Rose 2007).

5.5.2 Controls

In my first hypothesis, I proposed that long-term growth leads to more critical political attitudes while more recent growth enhances incumbent support. This is perhaps the most difficult hypothesis to test in the absence of longitudinal incumbent support data. At the same time, quarterly or even yearly growth changes are unlikely to significantly impact political attitudes, let alone political changes. Van Erkel and Van der Meer (2016, 194) speculate that unemployment

[21] Robustness checks: The question on United Russia reads: "Imagine that parliamentary elections are held next Sunday. What party would you vote for?" Alongside United Russia and six other parties, respondents have a choice of 'other party,' 'I would destroy the ballot," "I wouldn't vote," and "I'm having difficulty answering." The question on Medvedev is phrased identically to that on Putin. The question about the government reads: "Is the Russian government performing well or poorly? Is its performance in recent times improving, deteriorating or staying the same?" Respondent options: "well and improving," "well and staying the same," "well, but deteriorating," "poorly, but improving," "poorly and not changing," "poorly and deteriorating" and "difficulty responding"

has a short-term impact on political trust whereas people respond to economic growth in reference to a more "stable, historical standard." One way to assess the long and short term impacts of growth in Russia is to simply to calculate regional GDP per capita (in other words, gross regional product per capita or *GRP per capita*) in 2011 as a percentage change from GRP per capita in 2000 and 2008. This provides 11-year and 3-year growth windows. Growth rates take a plunge during the 2008 financial crisis, making the 2008-2011 growth period a proxy of how well regions recovered from the crisis.[22]

Assuming that regions which have experienced the most growth over 11 years are *different* from those which have experienced the most growth over 3 years, I would expect that regions with steep 11-year growth will have lower incumbent approval than regions with steep 3-year growth. If the same regions are demonstrating the highest growth in both time frames, however, one reason might be because of regional path dependence on economic structure. If regions which had historically specialized in competitive industries like fuels since the Soviet period fared best in the post-socialist transition, they might have also grown the most over the 2000s and fared best in the 2008 financial crisis. If we wish to separate out high growth regions in an 11 and 3 year period, it is important to control for initial regional GRP at the start of both growth periods.

In my third hypothesis, I proposed that growth rates will have weaker impacts on incumbent support in more politically democratic regions. To test this proposition it is possible to

[22] The regional GDP per capita data are reported by the Russian statistical agency in nominal prices, or in the prices of each year. It would be inappropriate to compare growth across different years with such data because they conflate real GDP changes with price fluctuation due to inflation. To obtain real GDP per capita growth estimates which account for inflation, I would ideally require the nominal GDP data in each region along with a regional price index like the GDP deflator or CPI. To my knowledge, no such regional price index is available. Granted this obstacle, I will assume that price changes in a given year are identical for all regions in the country and will use the national GDP deflator to convert nominal into real data at the regional level. GDP deflator data are available from World Bank. http://data.worldbank.org/indicator/NY.GDP.DEFL.ZS?locations=RU

assess differences in the growth effect at each growth period among politically open, hybrid and closed regions using the latest available regional democracy rankings in 2004 which summarize indicators of regional political pluralism, economic openness, civil society activism and press freedom.[23] Because regional democracy is fairly stable over time, small regional shifts in political openness over the previous decade are unlikely to significantly change where the regions stand in 2011, particularly if the regions are grouped into three broad categories of political openness.

I also control for regional dependence on the federal center and regional income inequality. The lack of growth in some parts of the country, particularly in blue collar, industrial rust belt regions, can be offset by federal transfers. Although federal transfers are no substitute for real growth, they can "fabricate winners and losers" in economic development (Popov 2001, 873-4).[24] I estimate income inequality by calculating differences in average monthly earnings per capita between the regions and Moscow in 2011 using data from the Russian State Statistics Service. This is a plausible measure in so far as residents of regions will be made aware of Moscow living standards on a regular basis through Moscow-based news reports on state news channels. Perceived differences between images of Moscow and their localities can lead to consternation with political authorities for failing to deliver similar economic performance at home.[25]

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[23] The full index combines a variety of indicators of regional democracy, including local balances of power, independence of local courts, violations of civil rights, the transparency of the political process, the competitiveness of local elections, the presence of stable, functional parties and coalitions, the independence of the press in the locale, corruption as measured by the political and economic power of local elites, the extent of economic liberalization, privatization and the protection of private property; the presence of civil society in the form of NGOs, referendums, independent activism and protests; elite turnover and local self-government in the form of elected bodies in community life. Full details are available from the Independent Institute of Social Policy at http://atlas.socpol.ru/indexes/index_democr.shtml

[24] Regional budgetary statistics in 2011 are publicly available from the Russian Ministry of Finance. Data available here: http://info.minfin.ru/region_compare.php

[25] Data available here: http://www.gks.ru/bgd/regl/B13_14p/IssWWW.exe/Stg/d1/04-02.htm

I will maintain regional democracy indicators as controls for each hypothesis, adding dummies for conflict ridden, repressed republics in the Northern Caucasus and the capital, Moscow. These regions have specific geopolitical characteristics which would likely skew incumbent support trends across the country. At the individual level I will maintain standard sociodemographic controls, including individual-level income. Unfortunately the survey does not contain information about respondents' exposure to state television, but it has a question about the last time respondents had used the internet. Responses can range from "in the previous 24 hours" to "more than a year ago." Frequent users are most likely to come across independent media sources; this question can act as proxy for one's ability to escape propaganda on state television.

Even with the necessary controls, I cannot prove that varying growth dynamics *cause* incumbent support to rise or fall. A few recent attempts to instrument economic growth in the former Soviet space would be unsuccessful in the present study. For example, Ananyev and Guriev (2016) treat the financial crisis of 2008 as an exogenous shock which allows them to isolate the causal influence of income changes on changes in interpersonal trust between 2008-2009. They instrument income change with the regional share of employment which produced industrial machinery, metal products, oil and gas in 1989 because these regions were still dependent on such industries in 2008 and were most severely hit by the financial crisis. While they make a case that this IV is exogenous to interpersonal trust, it would not be exogenous to incumbent approval because these industries came under heavy state manipulation as oil prices rapidly increased in the mid-2000s. People's experience with the state through their employment would affect their political attitudes through channels other than an income shock in 2008-2009.

De Haas et al. (2016) make a similar attempt to diagnose the causal effect of the economic crisis on attitudes toward democracy and the market economy in Ukraine, instrumenting income

change with the financial health of foreign banks in the country. Their rationale is that the crisis was channeled to Ukraine mainly through foreign owned banks; thus, poorly performing foreign branches would have left households more credit-constrained during the crisis. This is a weak design for several reasons, whether the outcome variable is attitudes toward democracy or incumbent approval. First, western banks are not the only transmitters of the financial crisis to households (as Ananyev and Guriev point out above), making it highly unlikely that the authors are isolating the treatment effect of crisis exposure. Second, banking in Russia and Ukraine is highly politicized. The top Russian banks (Sberbank and VTB) have more assets than the next 20 banks combined, meaning that people's banking experience is overwhelmingly tied to the state. Loan activity will also be correlated with the government's developmental agenda in different regions, making economic development across the country far from independent of political activity. Finally, even if bank health were the only transmitter of the crisis to households, we would have to argue that it affects incumbent support *only* through that transmission of the crisis. But banks with varying loan dependencies are not randomly distributed throughout the population. Healthier banks are likely selected into wealthier, urban and cosmopolitan regions where people are more likely to take out loans to start businesses—and also perhaps distrust the government.[26]

Other than these attempts, the number one instrument for income or income growth is rainfall (Bohlken and Sergenti 2010; Tanaka et al. 2010), but the rationale only works for agriculture-dependent countries (rainfall is expected to raise incomes by increasing crop yields). Rainfall would not be a plausible IV in a country like Russia, which is not an agriculture-dependent economy. More approaches are now avoiding implementing IVs for economic development in

[26] Both attempts to isolate a treatment effect depend on income *shocks* through the financial crisis. The causal influence of a shock is theoretically different from long-term economic growth, making such IVs inappropriate tests of the current hypotheses on both a theoretical and statistical level.

favor of explaining causal mechanisms in the absence of pseudo-experimental designs (Deaton 2010; Bazzi and Blattman 2014). Without a decent instrument for economic growth, I will have to continue with a correlational design with relevant controls. It is certainly true that people will have had many experiences with the state now and through history which will play a part in their incumbent approval, and which are not functions of growth. It is not possible to account for everything, but I have attempted to account for the most relevant confounding factors to isolate the interactions of interest.

5.6 Results

I run a binary probit model where Putin's approval is a factor of growth (as a percentage change in GRP per capita since 2000 or 2008), initial GRP per capita in 2000 or 2008, regional democracy, income inequality between the regions and Moscow, federal welfare transfers to the regions, and individual-level sociodemographic controls.[27] I test each interaction separately for clarity of interpretation, estimating changes in the predicted probability of approval across growth rates for different levels of democratic openness, as well as the average marginal effects of growth on the predicted probability of approval across the full range of initial GRP in both time intervals.

Descriptive statistics show that there is significant dispersion in both absolute GRP in 2011 and its growth over time across the country (Table 5.1). In 2011, there were five ultra-wealthy outliers (Moscow, Tumenskaya Oblast, Sakahlinskaya Oblast, Khanti-Mansiiskij Autonomous

[27] The model specification for an interaction between regional democracy and growth between 2000-2011 would look like:

$$\Pr(\mathrm{IA}_{ij}) = \beta_0 + \beta_1\mathrm{Growth}_j + \beta_3\mathrm{Dem}_j + \beta_4\mathrm{Growth}_j\mathrm{Dem}_j + \beta_2\mathrm{IGRP}_j + \beta_6\mathrm{Inequality}_j + \beta_7\mathrm{Transfer}_j + \beta_8\mathrm{Urbanity}_{ij} + \beta_9\mathrm{Internet}_{ij} + \beta_{10-11}\mathrm{RegDummies}_j + \alpha_{ij} + u$$

where IA = incumbent approval for individual i in region j, Dem= regional democracy, IGRP= initial GRP, Inequality= regional income inequality from Moscow, Transfer= federal transfers to the regions, RegDummies = regional dummies for Moscow and the Northern Caucasus, and alpha = vector of sociodemographic controls.

Okrug and Yamalo-Nenetskij Autonomous Okrug) on the GRP per capita distribution. Even if we ignore these outliers, the wealthiest region at the tail end of the distribution, the Republic of Komi, is still 4.65 times wealthier than the poorest region, Karachevo Cherkesskaya Republic. Regions near the median of the distribution are almost twice as rich (about 1.8 times) as those in the fifth percentile.

Growth patterns are different across the 11 and 3 year periods. We observe a significant long-term growth footprint across the country. Growth in GRP per capita between 2000 and 2011 has the highest dispersion, with 4 outlier regions which experienced ultra-high growth rates (Moscow Oblast, which grew 140.31 percent over this period, Belgorod Oblast at 143.82 percent, Dagestan at 169.54 percent and Sakhalin at 299.45 percent). The top 10 growing regions more than doubled GRP over this period, while the bottom 10 grew by less than 35 percent. Excluding outliers, the dispersion runs from GRP per capita growth as little as one percent to as large as 118 percent.

Regional GRP per capita growth in the 08-11 period is considerably smaller in dispersion. Due to the financial crisis, about half of the regions experienced a contraction in GRP per capita over this period. Excluding outliers, the dispersion runs from a GRP per capita contraction of 24 percent to a positive growth of 21 percent, with high-growth outliers only 27 and 29.6 percent (Sakhalin and Magadan, respectively). From this we can expect a minimal or null impact of short-run growth on approval.

14 **Table 5.1. Dispersion of Predictors of Interest**

	1st perct.	25th perct.	50th perct.	75th perct.	95th perct.
GRP 2011	105.5k r.	169.9k r.	224.4k r.	298.9k r.	865.6k r.
GRP Growth 00-11	1.09%	41.81%	64.79%	79.36%	118.64%

GRP Growth 08-11	-24.30%	-9.73%	-4.21%	4.37%	11.91%

Note: GRP 2011 is expressed in rubles at 2011 price levels. GRP growth in both ranges is measured in percent growth in regional GRP in 2011 either since 2000 or 2008.

Although the distribution is skewed with regards to both absolute economic performance and growth, the outliers are a unique feature of the Russian development landscape. I will keep all outliers in the dataset as indicative of real, extreme disparity in economic conditions across the country, looking at predicted probabilities of approval of Putin across the full range of these economic performance distributions.

A variance components analysis indicates that only 2.7 percent of variation in Putin's approval can be attributed to region-level characteristics, which illuminates the lack of a multilevel data structure. Substantively, this means that Putin's approval is quite uniform throughout the country regardless of regional clustering. Contrary to expectations, regional economic performance and growth will have less of an impact on approval than I anticipated. This is precisely what I find in the probit models below.

Predicted probabilities are estimated separately for each predictor while holding other predictors at their means, medians or modes.[28] Despite the marked variation in absolute GRP levels and long term growth within the country, none of these key variables has a meaningful effect on Putin's approval (Table 5.2). Absolute GRP in 2011 does not begin to significantly matter until the 99th percentile of the GRP distribution, which signals that approval ratings are marginally higher in outlier regions than the rest of the country, and there is no further internal contextual

[28] In other words, the effects of each predictor on approval are estimated for individuals of an average age, income, education, and frequent internet usage located in a region of moderate or high urbanity and median GRP, inequality, welfare transfers and democratic openness. Controls are maintained for Moscow and the Northern Caucasus republics.

variation in approval ratings. Growth does not have any significant impact on the predicted probability of approval at either the 10 or 3-year growth periods. These null effects are consistent across robustness checks to alternative dependent variables. If we consider these latter variables in their binary form (coding as '1' those who are pleased with and believe there is consistency or improvement in the performance of the person or party and '0' for all less enthusiastic evaluations), approximately 72 percent of the sample approves of Putin, 58 percent approves of Medvedev, 37 percent approves of the government in general, and 49 percent would vote for United Russia in a hypothetical election next Sunday rather than any other party, disposing of the ballot or not voting at all. Despite this variation in separate approval measures, less than three percent of variation in each measure is a function of region-level characteristics, which allows us to rule out any meaningful region-specific absolute GRP or growth effects.[29]

15 **Table 5.2 Predicted Probabilities of Approval for Predictors of Interest**

	1st perct.	25th perct.	50th perct.	75th perct.	95th perct.
GRP 2011	.708	.716	.723	.732	.796
95% CI	*.674-.742*	*.682-.749*	*.689-.756*	*.698-.765*	*.745-.846*
GRP Growth 00-11	.707	.715	.719	.723	.731
95% CI	*.675-.738*	*.684-.746*	*.684-.755*	*.684-.762*	*.680-.782*
GRP Growth 08-11	.718	.720	.721	.723	.724
95% CI	*.668-.768*	*.684-.756*	*.688-.754*	*.690-.755*	*.689-.759*

Note: These predicted probabilities are calculated holding all predictors at their means, medians or modes. For the effects of controls, see Table X. Standard errors are clustered at the region level.

Most other region-level characteristics also have little to do with Putin's approval (Table 5.3). Welfare transfers and regional democracy have no meaningful or statistically significant difference in effect across their ranges. With regards to income inequality, surprisingly, regions closest in income to Moscow have a *lower* predicted probability of approval than the rest of the

[29] The intraclass correlation (ICC) at the region level is 2.2 percent for approval of Medvedev, 1.6 percent for a determination to vote for United Russia, and 2.8 percent for approval of the government.

country—a difference of about 10 percentage points which is ultimately statistically insignificant at the 5 percent level. Substantively, the effect is mainly captured by Tumenskaya Oblast and Khanti Mansiiskij Okrug; the former region has jurisdiction over the latter, which produces the majority of Russia's oil. These regions are not representative of socioeconomic performance in the country.

16 **Table 5.3. Predicted Probabilities for Controls**

Continuous Predictors

	1st perct.	25th perct.	50th perct.	75th perct.	95th perct.
Transfers	(smallest transfers)				(largest transfers)
	.732	.726	.723	.718	.692
95% CI	*.697-.767*	*.693-.759*	*.689-.756*	*.682-.753*	*.628-.756*
Inequality	(most unequal)				(most equal)
	.745	.731	.723	.710	.652
95% CI	*.704-.786*	*.696-.766*	*.689-.756*	*.676-.744*	*.576-.729*
Regional Democracy	(least democratic)				(most democratic)
	.723	.724	.723	.721	.719
95% CI	*.687-.764*	*.692-.756*	*.689-.759*	*.680-.762*	*.659-.777*

Categorical Predictors

Sex	Male	Female			
	.602	.723			
95% CI	*.565-.639*	*.689-.756*			
Age	Age 20	Age 40	Age 60	Age 80	
	.769	.731	.689	.647	
95% CI	*.738-.799*	*.698-.763*	*.654-.726*	*.605-.688*	
Education	Secondary or Lower	Technical College	Some/full University+		
	.763	.723	.683		
95% CI	*.734-.792*	*.689-.756*	*.652-.715*		
Income	<4k rub	4-7k rub	7-10k rub	10-20k rub	20-45k rub
	.722	.687	.723	.726	.729
95% CI	*.689-.755*	*.651-.723*	*.689-.756*	*.699-.752*	*.694-.765*
Urban	Rural/Village	50-250k ppl	250k-1m ppl	1m ppl+	Moscow
	.792	.753	.723	.772	.772
95% CI	*.766-.817*	*.722-.783*	*.689-.756*	*.724-.819*	*.667-.877*
Internet	Never used	Used in last week or longer	Used in last 24 hrs		
	.740	.719	.723		
95% CI	*.711-.769*	*.686-.753*	*.689-.756*		
Protest 2011-12	Top 10	Bottom 90			
	.716	.725			
95% CI	*.647-.783*	*.695-.755*			

Note: Each effect is tested separately while holding the other predictors at their medians or modes. Controlled for 2011 GRP per capita (estimates presented in Table A). The probit estimations are run with clustered standard errors and 36,827 observations.

There are interesting, pronounced sociodemographic effects on approval. Perhaps the most distinct is the role of sex. The predicted probability of approval rises from 60 percent among men to 72 percent among women, a significant difference. Age also has a consistent negative effect on approval. Somewhat surprisingly, young people are significantly *more* approving of Putin than baby boomers and older generations. The predicted probability of approval decreases from 77 percent at age 20 to 65 percent at age 80, and drops progressively across the intermediate age range, with significant differences between under-30s and over-60s. Education carries a significant but modest corrosive effect. The predicted probability of approval rises from 68 percent among those with at least some university or a full postgraduate education to 76 percent among those with a secondary education at most.

Also of interest is the lack of an effect of urbanity. Residents of rural villages have an equivalent predicted probability of approval to residents of Moscow (79-80 percent), although the margin of error is higher in Moscow. Citizens of cities with 250,000 to one million people are significantly less approving of Putin than those in rural villages by seven percentage points. No other significant variation in approval is evident across other levels of urbanity. Internet usage also lacks an effect; there is no difference in approval among those who frequently use the internet, those who have used the internet in the last week or longer, and those who have never had to use the internet. It should also be noted that approval patterns do not map onto the protest patterns which occurred in the recent past or 10 months following this survey. Using Lankina's (2015, 33)

tabulations of top protesting regions between 2007-2012 targeting political, economic and social grievances, the top ten regions[30] do not differ from the rest of the country in approval patterns.

The interactions also bear little fruit. Regional democracy and growth in the 2000-2011 period have neither absolute nor interactive effects on approval, although growth tends to marginally boost approval more in highly *democratic* regions. Counterintuitively, the predicted probability of approval among residents in democratic regions rises from approximately 67 percent to 87 percent as we move from a growth rate of 1 percent to a growth rate of 170 percent. This boost in predicted probability is at least 10 percentage points higher than in less democratic regions at high levels of growth. It is also curvilinear, suggesting some diminishing political returns to growth: increases in the predicted probability of approval at small levels of growth taper off as the growth level steepens. Ultimately, however, the growth effect is insignificant between levels of sub-national democracy if we account for regionally clustered standard errors in the probit estimation. As expected, there is no such interactive effect in the 2008-2011 growth period either (Appendix C1-2).

I also hypothesized that we would observe an interaction effect between initial GRP levels in either 2000 or 2008 and regional growth rates since those periods. Neither of these effects takes place. I estimated average marginal effects for the influence of growth on the predicted probability of approval across levels of initial GRP, maintaining the standard sociodemographic controls. I expected to see a negative interaction (the growth effect should be smaller for initially wealthy regions), but it is substantively and significantly not different from zero. Again, predictably, the effect is also non-existent in the 2008-2011 growth period (Appendix C3-4).

[30] Moscow, St Petersburg, Samara, Moscow Oblast, Penza, Sverdlovsk, Voronezh, Primorsky Krai, Kaliningrad, Krasondar, Novosibirsk, Nizhegorodskaia, Chelyabinsk, Kirov, Ulyanovsk

Granted the null effect of absolute socioeconomic performance and growth on approval, do economic perceptions retain an influence? To consider this possibility I drew on three questions measuring pocketbook evaluations, regional sociotropic evaluations and retrospective pocketbook evaluations.[31] Unfortunately most of these variables are not scaled with enough nuance to capture enough observations across their range; only 6.45 percent of the sample marked that their personal financial situation is "really good" or "good," 4.97 percent marked that their regional economic situation is "good" and 10 percent marked their situation as having improved over the past. Even with this limited scale and variability, we can still make some observations. First, each of these variables has a meaningful positive influence on approval which is significantly higher across each subsequent category despite the low number of observations at the top. Moving from evaluations that one's personal material situation is "really bad" to "really good", the predicted probability of approval increases from 53 percent to 90 percent. Moving from bad to good evaluations of regional economic performance, the predicted probability of approval increases from 59 percent to 92 percent. Moving from those who believe their personal material situation has deteriorated to those who believe it has improved over the previous year, the predicted probability increases from 62 percent to 86 percent.

17 **Table 5.4 Predicted Probabilities of Approval for Perceptions Predictors**

Pocketbook	Really Bad	Bad	Medium	Good	Really Good
	.527	.645	.751	.838	.902
95% CI	*.477-.574*	*.606-.683*	*.719-.783*	*.809-.866*	*.876-.927*
Regional Sociotropic	Bad	Medium	Good		
	.594	.789	.915		
95% CI	*.554-.634*	*.758-.821*	*.894-.936*		
Retrospective	Worse	The Same	Better		
	.617	.750	.854		

[31] Pocketbook: "How would you evaluate your current material situation" on a 5-point Likert scale from "very bad" to "very good"; Regional Sociotropic: "How would you currently assess the state of the economy in your region—good, medium or bad?"; Retrospective Pocketbook: "What do you think, has your material situation in the last year improved, deteriorated or stayed the same?"

95% CI	*.577-.656*	*.722-.779*	*.833-.876*

Note: These predicted probabilities are calculated holding all predictors at their means, medians or modes.

As a second observation, there are no meaningful correlations between any relatively objective measures and subjective perceptions of economic performance. Growth is not associated with pocketbook evaluations (correlation= -0.02), regional sociotropic evaluations (-0.03) or retrospective evaluations of the economy (-0.02). Absolute GRP across regions in 2011 is also unrelated to these perceptions (with respective correlations at -0.03, -0.09 and -0.03).

It remains puzzling that economic perceptions are not linked to real performance. What explains this gap? Growth alone boosts neither incumbent approval nor perceptions of economic performance. One limited hypothesis we could test is that people may be prone to believe they are doing better when they also have some political freedom. Residents of more repressive regions may in turn be more likely to discount official news, statistics or notions that their lives are better than they actually are. Unfortunately there is no easy way to test this proposition with the coarsely scaled perceptions data at hand. If we take the perceptions variable with the most variation at the top of its scale, retrospective pocketbook evaluations, we find that growth enhances the predicted probability of reporting a positive retrospective evaluation of one's material status, but again, this estimate is not significantly different from more repressive regions (Appendix C5). This null effect is likely an artifact of poor data and should be re-examined in empirical contexts with more differentiated survey scales of economic perceptions.

In the absence of longitudinal approval data, it can still be possible that real growth has had a cumulative positive influence on approval over time. At present, however, the lack of an effect at two different time lags is consistent with other survey-based literature which has turned out ambivalent or null effects of objective economic indicators on approval over time.

Discussion

Contrary to my expectations, Putin's approval rating was not vulnerable to internal variation in absolute levels of socioeconomic performance or long-term growth months prior to the largest protest breakout in the country's post-Soviet period. Despite substantial variation in economic performance across the country over time, it has been of little consequence for Russia's reigning incumbent. It would appear from this analysis that Putin has significantly more control over the long-run post-material effects of economic development which have purportedly brought down long-term autocracies in the third wave of democratization. On the other hand, such results might only signify the limits of what we can learn about regime stability from the study of public opinion. If the standard economic theory does not fit, what are the real foundations of incumbent approval in hybrid authoritarian regimes like Russia, and why do the approval patterns fail to map onto real manifestations of dissidence?

There remains a gap between literatures on regional politics, public opinion and social movements which deserves exploration. Examining the implications of regional diversity in Russian socioeconomic performance, Zubarevich (2012) argues that "the increasing concentration of active and well educated citizens in the big cities tolls the death knell of the 'power vertical.'" She speaks to the concentration of internet users in large cities, whose desire to protest in 2011-2012 was "not an economic crisis or a collapse in the price of oil, but rather a moral repugnance." Lankina (2015) connects Zubarevich's regionalism argument to social movement theory. Using the same regional democracy index as I use in this paper, Lankina (2015, 28) argues that high-democracy regions with greater pluralism in civil and political life "retained their edge as entities

contributing to the generation of uncertainty faced by the federal electoral authoritarian regime, much more so than did the more docile regions that continued to engineer grotesquely large margins of victory and turnouts in federal elections." She documents that most protests in 2011-2012 occurred in what Zubarevich classifies as Russia 1 (cosmopolitan large cities) or Russia 2 (industrial cities motivated by socioeconomic concerns) as opposed to Russias 3-4.

Granted the above data analysis, however, no regional characteristics predict the population's approval of Putin, Medvedev, United Russia or the broader government prior to the protest wave. Russias 1-4 are on equal standing with regards to perceptions of Putin. Moscow is equivalent in approval to the poorest regions in the country. Docile regions are as supportive of Putin and company as the most liberated and plural. The usual suspects behind democratic flourishing in modernization theory—education, income, urbanity, internet usage and youth—have almost consistent null effects on approval of all of the above, with the youth being more pro-Putin and education exerting a very modest corrosive effect (the predicted probability of approval of Putin remains at 68 percent among the highest educated strata in the sample). Contrary to Zubarevich's argument, this collection of regional and individual factors does not carry the seeds of dissidence or forebode the death knell of the "power vertical." What explains the simultaneous uniformity of approval and the diversity of anti-regime social mobilization across the country?

Smyth (2014, 570) argues that trust in Putin is tied to personalism, a "composite mixture of a charismatic myth, effective state management, and a symbolic ideology that portray Mr. Putin as the source of Russian stability and architect of Russian revival…" In her analysis of the 2011-12 protests, she argues that rival narratives and opinion data in the pro- and anti-Putin rallies illuminate a "clear attitudinal and behavioral divide within the Russian population" (ibid., 585). Those on Putin's side defended his role as a steward of both state and economy while anti-Putin

protesters aimed to undermine the personalism of such appeals. But much like Zubarevich's and Lankina's assessments, claims about fissures in public dispositions toward the state within the country should be moderated. Given the near uniformity of support for Putin in what appeared to be the most vulnerable point of his tenure, we should not miss the forest for the trees by inferring population characteristics from the vantage point of the protests.

At the same time, this latter point depends on our object of inquiry. Protests are better indicators of *political legitimacy* than approval ratings in so far as they indicate citizens' unwillingness to accept the authorities as rightful holders of power. Demonstrations of public dissidence erode the veneer of the regime's invincibility, helping citizens imagine an alternative universe to the status quo. The implications of such anti-regime acts on the public imagination can be crippling for the incumbent, however small or geographically confined they start out. Responding to the latest wave of anti-Putin protests in early February 2017, Sam Greene argues that Putin's approval ratings are not a matter of fear but people's conviction that they are

> living in a country where there were no alternatives, and thus no choices to be made, a country in which society could not be pushed to act on its own behalf, in which solidarity extends no further than rhetoric… Imagination matters in politics. When dealing with the past and present, we can hope to rely on fact… The future, however, is always imagined. What happened today demonstrated the imagination watershed in Russia, between those who cannot imagine a future much better than today, and those who can. That's why the Kremlin went hard at Navalny and the protesters today; to restore the confidence of the unimaginative.[32]

Indeed, we find in my analysis that the economic imagination matters more for approval than economic circumstances. Regardless of one's current or past wellbeing, the past can be re-imagined and a better future can always be anticipated. The fact that young people in their twenties

[32] This came from a blog post in the immediate aftermath of the April 2017 protests: https://moscowonthames.wordpress.com/2017/03/26/game-on/. Alexey Navalny is an oppositional leader and prospective 2018 presidential candidate who incited the latest anti-corruption protest wave.

are more likely to approve of Putin by 10-12 percentage points more than their grandparents is also striking. These people were too young to remember the 1990s or the Soviet Union, historical comparisons which are part and parcel of the state's narrative about stability under Putin. Their allegiance to the regime is less likely a function of bad memories as positive expectations of the future. In this sense, Smyth's analysis of the different narratives about regime legitimacy between pro- and anti-Putin protesters is important. Political legitimacy in Russia will likely depend more on who ends up controlling this narrative and less on economic conditions. The Kremlin's investment into an expansive propaganda machine is consistent with this rationale (Pomerantsev 2014) and helps explain Putin's sustained approval ratings despite a heavy recession compounded by sanctions between 2014 and 2016.

Interestingly, using such data, we could not have predicted the latest protest wave in April 2017, which was populated largely by young students across the country. Their ability to imagine an alternative future cannot be reaped from individual sociodemographic characteristics or structural variables at the country or region level. Social mobilization is not linearly or randomly distributed. Personal histories, network effects and triggers interact with the environment in complex ways which require mixed methodologies to uncover. The discovery of how competing imaginations emerge and spread will say more about the vulnerabilities of hybrid political regimes like Russia than brute facts about economic structure. In this way, static approval ratings and dynamic social mobilization patterns are empirically compatible phenomena. The uniformity of incumbent approval can be a chimera, something which is susceptible to sudden shifts in ways of thinking about the political future. My data analysis in this chapter speaks to the weakness of popular theories about the influence of economic growth and regional characteristics on Putin's

popularity, but we can ultimately infer little from such data about the foundations of political legitimacy in Russia.

6 Conclusion

This dissertation combines commentaries on the methodology of cross-national public opinion research and the comparative political economy of political trust formation across the regime spectrum. I have attempted to shed light on how economic incentives shape political beliefs and corruption responsiveness in understudied parts of the former Soviet space. Why do governments known for their corruption maintain high levels of political trust in Latin America, China and Central Asia? Do autocrats and democrats alike use economic tools to muffle corruption resentment and depoliticize society? Does economic development serve as an opiate for the masses in corrupt systems of governance? How can we best compare sensitive attitudes across open and closed societies? My research makes very modest inroads into these questions, focusing on macro-level theories and cross-national survey data in the former Soviet space while leaving immense room for targeted theory building about causal mechanisms, exploration of novel case studies and methodological innovation in the study of political trust around the world.

The expansion of survey data into developing and authoritarian countries in the last decade has complicated our understanding of what it means to trust a political institution, what constitutes corruption and how to compare these concepts intelligibly around the world. In the first empirical chapter of this dissertation (Chapter 2), I use multiple group confirmatory factor analysis, a subset of structural equation modeling, to test the cross-national validity and measurement equivalence of political trust. I show that averaging sets of standard survey questions using a "kitchen sink" approach can mask significant differences in how people interpret those questions across the regime spectrum. Mostly due to data limitations, this test of measurement equivalence does not fully address mounting concerns about the gap between concept building and operationalization in the political trust literature. That is, I do not begin with a sophisticated, detailed account of

political trust and test its validity and equivalence across the countries in the survey data. I show, however, that naïve index building even with the simplest questions about institutional trust can make for a clumsy mean comparison in cross-regime contexts. Building survey questions around more sophisticated concepts is a needed effort, but requires sufficient care to avoid idealism in measurement building which can distort the validity and comparability of survey questions in different contexts. Perhaps the best way to reconcile the largeness of the N and the richness of the locale in measurement building is to conduct meaningful ethnographic research and probing studies which capture local knowledge about the concept under study and allow the researcher to parse out elements of measurements which can and cannot be generalized across societies. Probing studies can incorporate small and large sample sizes. With small samples, researchers can conduct focus groups across populations in which respondents are asked to react to different question wording and phrasing. With larger samples, researchers can ask open-ended questions about what sensitive constructs (like political legitimacy or corruption) mean to people in different populations, and one can probe such narratives using quantitative text or discourse analysis (see Braun et al. 2014 for a comprehensive literature overview). Opportunities for such detailed work are ripe, as public opinion research in developing and authoritarian countries is still very young.

In the body of my thesis (Chapters 3 and 4), I examined the applicability of the theory of corruption tradeoff in the former Soviet space, where citizens of some autocracies have extraordinarily high levels of trust in their political institutions despite registering the highest petty corruption perceptions in the region. Popular theories of corruption tolerance in Latin America suggest that citizens hold politicians less accountable for corruption in good economic times or when they are the beneficiaries of short term welfare transfers. I examined the moderating influence of different indicators of economic satisfaction and flourishing on how citizens most

affected by petty corruption trust their presidents and political institutions in the former Soviet space. I found little evidence, however, to suggest that citizens in former Soviet countries trade off concerns about corruption when they are (or believe they are) prospering at an individual, regional or national level. In fact, high corruption perceivers in among the most politically trusting countries in the sample, Azerbaijan and Uzbekistan, demonstrated the *lowest* trust in their political institutions. Citizens of autocracies in general tend to reward political institutions more for "goods" (like economic performance) and punish them more for "bads" (like corruption) than their counterparts in democracies, although there is interesting variation in corruption responsiveness among Central Asian autocracies that deserves future study. It would be incorrect to discard such high-trust data among authoritarian countries on the assumption that respondents are too afraid to reveal their true opinions. Granted a scatterplot of the cross-country relationship between corruption and trust, it would also be an ecological fallacy to infer individual-level characteristics from cross-national aggregates of public opinion. Theories about preference falsification or cultural path dependence on petty corruption do not stand up to test when we observe that citizens in among the most bribe-dependent cultures in the sample respond overwhelmingly negatively to such abuses when rating their political institutions.

The puzzle of these chapters was ultimately about the concurrence of high trust and high *petty* corruption (rather than grand corruption), making it an imperfect test of the external validity of corruption tradeoff theory. While my data did not allow me to check the robustness of my results to grand corruption measures (due to their absence from the survey), I was able to replicate a study by Zechmeister and Zizumbo-Colunga (2013) on corruption tradeoff in Latin America, finding that the sign of the interaction flips when we replace grand with petty corruption perceptions. Consistent with my own results, petty corruption perceivers who are increasingly satisfied with

their economic conditions progressively *downgrade* their trust in political institutions. Economic improvements appear to magnify rather than mitigate the influence of direct extortion on political attitudes. This presidential penalty does not appear to have any relation to postmaterial values as measured by critical orientations toward authority, preferences for democracy or elections, or the moral condemnation of various corrupt practices. The results to such sensitivity checks suggest that economically satisfied corruption perceivers are not necessarily exercising more sophisticated notions of political accountability or employing stricter standards for governance than their less satisfied counterparts. Being fed up with corruption appears not to require a liberal or postmaterial attitudinal basis.

The findings in this section also offer an avenue for testing the causal influence of different *kinds* of corruption on political attitudes with survey or field experiments in democratic and non-democratic contexts. The implications of such extended research can be useful in understanding reform traps in clientelist democracies and autocracies: on the one hand, people might be willing to overlook large scale corruption; on the other, constant exploitation at the hands of government-run schools, hospitals or bureaucracies instigates dissatisfaction with the authorities which only gets worse in boom times. One set of mechanisms demobilizes society while the other mobilizes it. One can help politicians get away with large scale malfeasance while the other might encourage citizens to hold political institutions more accountable for institutional dysfunction. Understanding this interesting push and pull can inform literatures on the political economy of development and transition about the causes of bottom-up pressure for reform in corrupt systems of governance.

In Chapter 5, I continued my exploration of the economic influence of political approval in a single case study, leaving behind perceptions indicators and focusing on the influence of more objective measures of absolute socioeconomic performance and growth across the diverse Russian

territory. Despite significant variation in economic performance and institutional openness across the country, these factors are not linked to citizens' support of Putin. Modernization theory from the 1960s and 70s would suggest that economic development gives rise to democratization in the long term. Yet large scale societal enrichment is not straightforwardly tied to attitudinal shifts or demands for change in modern autocracies like Russia and China. Both regimes have demonstrated resilience to any potentially liberalizing consequences of economic growth, engaging in forms of authoritarian entrepreneurship to control the narratives that guide popular imaginations about their political futures (Pomerantsev 2016, Brady 2016). The study of this narrative control will likely prove to be more informative about authoritarian political legitimacy than economic structure. The incumbent approval data I used in this chapter could not have predicted the imminent outbreak of the largest anti-regime protests in Russia's post-Soviet history, nor their geographical distribution. Survey data analysis does not adequately tap into the systematic, non-linear influences on people's motivations to challenge regime-led narratives. Social network analysis, process tracing and ethnographic work can crucially supplement survey data to uncover the content of such motivations. By moving beyond structural arguments and illuminating the role of the political imagination, we can better understand the foundations of authoritarian resilience and vulnerability in the twenty-first century.

Political trust is an incredibly important binding force between governors and governed, both in its benign capacity to support democratic processes and its pernicious capacity to advance authoritarianism. Understanding the incentives and notions which undergird this trust can illuminate why some political systems change and others survive. In his most recent work, Granovetter (2017, 75-79) reviews notions and applications of trust in social and economic life, arguing that the term is relevant to far more than proximate relationships between individuals. He

argues that Hardin's view about the necessity of personal familiarity and interest encapsulation for trusting relationships to work would recommend too mechanistic and flawed a view of social cooperation in which all manner of interactions in public (non-intimate) life would have to be seen as the result of rules, sanctions or path dependent norms. This is typically not the case in small communities, market settings or large political societies. I believe Granovetter is correct in arguing that "it is more fruitful to theorize at both small- and large-scale levels under what circumstances people assume that others in a position to hurt their interests will not do so" (ibid., 80, my italics). Trust is conceptually relevant to a variety of empirical settings. Indeed, when it comes to politics, trustees in power are in a position to cause immense potential harm to their truster-constituents. Constituents' expectations and attitudes toward these actors are far from irrelevant in the social science research landscape on trust. As we obtain better opinion data in hard to reach empirical settings across the world, we will develop a better understanding of the ties that bind us not only to each other, but to the political systems that help determine our ability to live well and freely. This dissertation is but a small step forward in this exciting research program.

Appendices

Appendix A (Ch. 2)

1) Fit Statistics for Models 1-4
2) Standardized factor loadings for each model
3) Table of trust means per country

1) Global Fit Statistics for each model

Fit statistics: Model 1 Regional and Local Political Trust

Country	Chi Square	RMSEA	CFI	SRMR	Reg & Loc Error Corr (SE)	Modification Index (EPC), standardized
Azerbaijan	27.055	0.076	0.993	0.013	.199 (.047)	
Turkey	54.334	0.112	0.979	0.023	.252 (.043)	REG WITH GOV 42.165 (.344)
Tajikistan	116.876	0.168	0.966	0.026	.298 (.055)	REG WITH GOV 112.105 (.644) LOC WITH GOV 60.538 (-.370)
Belarus	55.132	0.12	0.986	0.015	.321 (.046)	PARL WITH REG 46.304 (-.421) REG WITH GOV 46.037 (.712)
Uzbekistan	63.597	0.103	0.991	0.012	.323 (.035)	PARL WITH REG 31.897 (-.266) REG WITH GOV 31.484 (.273)
Bosnia	92.748	0.144	0.983	0.015	.329 (.038)	REG WITH GOV 83.420 (.512) PARL WITH REG 40.389 (-.363)
Ukraine	42.524	0.079	0.991	0.013	.388 (.027)	REG WITH GOV 32.756 (.277)
Russia	96.368	0.122	0.979	0.021	.422 (.029)	REG WITH GOV 79.976 (.337)
Kyrgyzstan	117.887	0.169	0.945	0.045	.435 (.045)	POLP WITH PARL 98.104 (.422) REG WITH GOV 65.924 (.417)
France	10.609	0.04	0.995	0.012	.447 (.027)	
Estonia	25.617	0.074	0.98	0.023	.455 (.033)	
Sweden	6.946	0.029	0.998	0.009	.455 (.034)	
Serbia	73.822	0.108	0.984	0.015	.478 (.025)	REG WITH GOV 71.166 (.303) LOC WITH GOV 42.352 (-0.201)
Germany	21.836	0.066	0.994	0.013	.495 (.028)	
Czech Republic	21.142	0.065	0.992	0.018	.515 (.025)	

Kazakhstan	22.489	0.07	0.995	0.011	.516 (.030)	
						POLP WITH PARL 33.648 (.282)
Armenia	45.584	0.105	0.988	0.015	.522 (.029)	REG WITH GOV 31.635 (.319)
Slovakia	13.838	0.05	0.996	0.01	.529 (.026)	
Italy	30.85	0.08	0.991	0.013	.531 (.024)	REG WITH GOV 31.029 (.230)
Moldova	30.485	0.08	0.993	0.009	.557 (.026)	
Mongolia	29.031	0.080	0.988	0.019	.569 (.028)	
Lithuania	12.378	0.046	0.995	0.015	.580 (.026)	
Georgia	15.397	0.055	0.997	0.011	.594 (.028)	
						POLP WITH PARL 36.549 (.391)
Bulgaria	57.393	0.115	0.978	0.026	.595 (.025)	POLP WITH GOV 32.086 (-.313)
						REG WITH GOV 91.411 (.449)
Albania	113.962	0.163	0.957	0.029	.598 (.023)	PARL WITH REG 70.629 (-.296)
						POLP WITH PARL 51.173 (.317)
Poland	72.349	0.104	0.986	0.019	.638 (.017)	REG WITH GOV 45.010 (.180)
Romania	43.027	0.096	0.984	0.017	.665 (.018)	REG WITH GOV 30.630 (.174)
Croatia	26.129	0.074	0.993	0.012	.707 (.017)	

Note:
All correlations are significant ($p<0.01$)
REG= regional government; LOC=local government, PARL=parliament, GOV=government, POLP=political parties. WITH = suggestion of an error correlation.

Fit Statistics: Model 2 Political and Protective Trust (Bi-dimensional model)

Country	Chi Square	RMSEA	CFI	SRMR	Factor Corr	Modification Index (EPC), standardized
Italy	9.44	0.036	0.998	0.014	0.327 (.032)	
Great Britain	3.519	0	1	0.006	0.372 (.040)	
Romania	6.375	0.024	0.999	0.014	0.418 (.032)	
Kosovo	26.81	0.073	0.991	0.013	0.443 (.030)	
Czech Rep	24.379	0.071	0.990	0.024	0.473 (.033)	
Croatia	10.933	0.042	0.997	0.012	0.491 (.031)	
Turkey	11.895	0.044	0.995	0.015	0.503 (.034)	
Slovakia	22.731	0.068	0.991	0.02	0.514 (.030)	
						PROTECT BY POLP 50.043 (.206)
Bosnia	90.723	0.142	0.974	0.042	0.558 (.025)	PARL WITH GOV 50.038 (2.011)
Latvia	17.92	0.059	0.988	0.017	0.558 (.038)	
Slovenia	35.172	0.089	0.981	0.024	0.561 (.032)	
Poland	17.315	0.046	0.996	0.012	0.562 (.023)	
Serbia	8.506	0.027	0.999	0.008	0.572 (.023)	

Lithuania	18.094	0.059	0.988	0.023	0.573 (.040)	
Germany	16.202	0.054	0.993	0.017	0.592 (.035)	
Mongolia	9.704	0.038	0.995	0.013	0.595 (.037)	
Estonia	26.477	0.075	0.978	0.024	0.605 (.038)	
France	3.479	0	1	0.009	0.610 (.031)	
						PROTECT BY POLP 40.678 (.237)
Hungary	42.114	0.096	0.985	0.032	0.624 (.025)	PARL WITH GOV 40.690 (983)
Sweden	14.107	0.053	0.988	0.021	0.638 (.044)	
Albania	14.291	0.05	0.993	0.018	0.644 (.032)	
Bulgaria	28.084	0.078	0.987	0.022	0.650 (.029)	
Georgia	6.591	0.026	0.999	0.013	0.665 (.024)	
Macedonia	18.142	0.058	0.994	0.016	0.683 (.024)	
						POL WITH POLP 45.759 (.389)
Belarus	52.419	0.114	0.973	0.037	0.694 (.029)	PROTECT BY POLP 36.044 (.331)
						PROTECT BY POLP 31.092 (.250)
Ukraine	33.387	0.069	0.990	0.021	0.727 (.023)	PARL WITH GOV 31.078 (.626)
						PROTECT BY POLP 37.570 (.267)
Moldova	45.722	0.101	0.986	0.027	0.731 (.024)	PARL WITH GOV 37.544 (1.170)
Armenia	20.414	0.065	0.992	0.015	0.733 (.030)	
Kyrgyzstan	23.286	0.07	0.984	0.022	0.747 (.030)	
						PROTECT BY POLP 24.899 (.245)
Montenegro	46.607	0.105	0.985	0.027	0.767 (.019)	PARL WITH GOV 24.886 (.841)
Russia	18.488	0.048	0.995	0.012	0.776 (.021)	
						PROTECT BY PARL 28.542 (-.422)
Tajikistan	43.479	0.099	0.982	0.026	0.786 (.021)	POLP WITH GOV 28.536 (-.275)
						PROTECT BY POLP 64.943 (.501)
Kazakhstan	65.451	0.127	0.973	0.036	0.790 (.023)	PROTECT WITH GOV 64.942 (1.233)
						POLP WITH GOV 43.184 (.519)
Azerbaijan	56.406	0.115	0.977	0.028	0.798 (.025)	PROTECT BY GOV 43.180 (.382)
						PROTECT BY POLP 121.964 (.510)
Uzbekistan	178.838	0.175	0.961	0.046	0.809 (.014)	PARL WITH GOV 121.952 (1.686)

Note:

All correlations are significant ($p<0.01$)
PARL=parliament, GOV=government, POLP=political parties, PROTECT = protective institutional trust factor, POLIT = political trust factor. WITH = suggestion of an error correlation. BY = suggestion of an indicator loading on the specified factor.

Fit Statistics: Model 3 Courts and Police Error Correlation

Country	Chi Square	RMSEA	CFI	SRMR	Error Corr	Modification Index (EPC), standardized
Kosovo	42.680	0.094	0.984	0.019	-0.050 (.035)	COURTS WITH POLP 38.429
Albania	46.761	0.102	0.976	0.024	0.056 (.036)	COURTS WITH GOV 33.882 (-.271)
Georgia	91.029	0.149	0.964	0.036	0.089 (.039)	COURTS WITH POLP 62.319 (.309)
Mongolia	13.936	0.050	0.992	0.015	0.104 (.037)	
Tajikistan	54.436	0.112	0.978	0.026	0.135 (.045)	
Azerbaijan	53.178	0.111	0.984	0.021	0.149 (.042)	POLICE WITH GOV 38.830 (.241)
Macedonia	45.438	0.099	0.980	0.021	0.154 (.035)	POLICE WITH GOV 37.244
Montenegro	48.297	0.107	0.986	0.019	0.198 (.037)	PARL WITH GOV 32.852 (.640)
Estonia	19.052	0.062	0.985	0.021	0.204 (.037)	
Italy	51.661	0.107	0.875	0.025	0.209 (.031)	COURTS WITH POLP 34.431 (.203)
Bulgaria	80.042	0.138	0.959	0.034	0.236 (.035)	PARL WITH GOV 47.837 (.544)
France	26.188	0.074	0.983	0.018	0.242 (.033)	
Croatia	20.695	0.065	0.992	0.015	0.251 (.032)	
Latvia	6.581	0.025	0.998	0.012	0.266 (.034)	
Britain	7.865	0.026	0.998	0.009	0.280 (.025)	
Turkey	25.545	0.073	0.986	0.021	0.280 (.033)	
Romania	22.427	0.066	0.990	0.018	0.284 (.031)	
Sweden	18.443	0.063	0.984	0.023	0.284 (.033)	
Belarus	65.173	0.128	0.970	0.043	0.285 (.034)	
Armenia	26.308	0.076	0.992	0.017	0.291 (.039)	PARL WITH GOV 56.504 (1.856)
Serbia	12.496	0.038	0.997	0.009	0.295 (.026)	
Kyrgyzstan	23.271	0.069	0.984	0.021	0.298 (.036)	
Kazakhstan	102.756	0.161	0.963	0.038	0.351 (.034)	PARL WITH GOV 105.577 (1.085)
Lithuania	21.023	0.065	0.987	0.024	0.356 (.030)	
Czech Rep	61.840	0.120	0.971	0.032	0.375 (.029)	PARL WITH GOV 58.853 (.899)
Poland	26.718	0.060	0.993	0.013	0.376 (.024)	
Russia	28.776	0.063	0.992	0.017	0.380 (.027)	
Slovakia	31.169	0.082	0.988	0.022	0.395 (.029)	
Moldova	59.483	0.116	0.983	0.028	0.407 (.030)	PARL WITH GOV 41.269
Hungary	42.901	0.097	0.984	0.026	0.426 (.028)	
Ukraine	40.486	0.077	0.989	0.023	0.429 (.023)	PARL WITH GOV 34.879
Bosnia	100.788	0.150	0.971	0.035	0.432 (.026)	PARL WITH GOV 91.344 (1.721)
Germany	8.843	0.034	0.998	0.013	0.440 (.026)	
Uzbekistan	236.213	0.202	0.951	0.052	0.496 (.023)	PARL WITH GOV 172.738 (1.964)

All correlations are significant ($p<0.01$)
PARL=parliament, GOV=government, POLP=political parties, WITH = suggestion of an error correlation.

Fit statistics: Model 4 (Simple)

Country	Chi Square	RMSEA	CFI	SRMR	Modification Index (EPC), standardized
Albania	15.208	0.080	0.991	0.018	
Armenia	9.499	0.063	0..997	0.010	
Azerbaijan	7.280	0.051	0.997	0.010	
Belarus	1.499	0.000	1.000	0.004	
Bosnia	15.152	0.078	0.996	0.009	
Bulgaria	39.545	0.137	0.975	0.024	PARL WITH LOC 33.975 (-.326) POLP WITH GOV 33.983 (-.373)
Croatia	5.515	0.042	0.998	0.008	
Czech Rep	13.745	0.076	0.992	0.017	
Estonia	2.085	0.007	1.000	0.010	
France	2.362	0.013	1.000	0.008	
Georgia	3.063	0.024	1.000	0.007	
Germany	15.686	0.081	0.993	0.013	
Great Britain	3.173	0.020	1.000	0.006	
Hungary	24.328	0.104	0.989	0.020	PARL WITH GOV 22.975 (-0.667) POLP WITH LOC 22.967 (-0.178)
Italy	0.541	0.000	1.000	0.003	
Kazakhstan	9.819	0.064	0.996	0.009	
Kosovo	34.128	0.122	0.986	0.019	LOC WITH GOV 34.900 (0.266) POLP WITH PARL 34.903 (0.425)
Kyrgyzstan	53.980	0.162	0.946	0.036	LOC WITH GOV 54.637 (.402) POLP WITH PARL 54.602 (.467)
Latvia	9.849	0.063	0.992	0.015	
Lithuania	4.604	0.036	0.997	0.011	
Macedonia	1.808	0.000	1.000	0.005	
Moldova	7.092	0.050	0.998	0.006	
Mongolia	9.581	0.062	0.994	0.015	
Montenegro	47.674	0.154	0.982	0.018	PARL WITH LOC 38.086 (-.404) POLP WITH GOV 38.083 (-.115)
Poland	24.925	0.085	0.992	0.015	LOC WITH GOV 23.558 (.194) POLP WITH PARL 23.558 (.266)
Romania	5.542	0.041	0.997	0.011	
Russia	9.817	0.050	0.997	0.010	
Serbia	2.136	0.007	1.000	0.004	
Slovakia	5.179	0.040	0.998	0.008	
Slovenia	7.534	0.053	0.995	0.012	
Sweden	2.971	0.023	0.999	0.008	
Tajikistan	7.280	0.051	0.997	0.010	
Turkey	13.177	0.075	0.993	0.015	
Ukraine	4.193	0.027	0.999	0.007	

Uzbekistan	27.013	0.094	0.994	0.012

Note: PARL=parliament, GOV=government, POLP=political parties, LOC= local government, WITH = suggestion of an error correlation.

2) Standardized factor loadings for each model

STANDARDIZED FACTOR LOADINGS Model 1 Trust in political institutions in relation to trust in local and regional government

Country	Trust in Government	Trust in parliament	Trust in Political Parties	Trust in Local Government	Trust in Regional Government
Azerbaijan	0.797 (0.014)	0.897 (0.009)	0.766 (0.015)	0.871 (0.011)	0.840 (0.013)
Turkey	0.817 (0.014)	0.819 (0.015)	0.521 (0.026)	0.780 (0.017)	0.803 (0.016)
Tajikistan	0.840 (0.012)	0.842 (0.013)	0.587 (0.024)	0.852 (0.013)	0.908 (0.010)
Belarus	0.945 (0.006)	0.863 (0.010)	0.549 (0.026)	0.837 (0.013)	0.933 (0.007)
Uzbekistan	0.925 (0.005)	0.920 (0.005)	0.726 (0.014)	0.883 (0.007)	0.927 (0.005)
Bosnia	0.916 (0.006)	0.919 (0.006)	0.723 (0.016)	0.859 (0.009)	0.933 (0.006)
Ukraine	0.877 (0.009)	0.841 (0.010)	0.694 (0.015)	0.629 (0.018)	0.836 (0.010)
Russia	0.809 (0.012)	0.879 (0.010)	0.668 (0.017)	0.773 (0.013)	0.842 (0.011)
Kyrgyzstan	0.708 (0.021)	0.667 (0.025)	0.573 (0.028)	0.742 (0.023)	0.824 (0.021)
France	0.735 (0.021)	0.803 (0.020)	0.591 (0.025)	0.399 (0.031)	0.548 (0.027)
Estonia	0.733 (0.025)	0.832 (0.025)	0.475 (0.031)	0.386 (0.032)	0.539 (0.034)
Sweden	0.785 (0.020)	0.808 (0.021)	0.611 (0.025)	0.687 (0.024)	0.687 (0.024)
Serbia	0.855 (0.010)	0.880 (0.009)	0.722 (0.014)	0.696 (0.015)	0.784 (0.013)
Germany	0.893 (0.010)	0.838 (0.012)	0.649 (0.020)	0.708 (0.018)	0.809 (0.013)
Czech Republic	0.837 (0.014)	0.870 (0.013)	0.700 (0.019)	0.468 (0.027)	0.613 (0.023)
Kazakhstan	0.918 (0.008)	0.872 (0.010)	0.692 (0.020)	0.806 (0.014)	0.857 (0.011)
Armenia	0.921 (0.008)	0.898 (0.009)	0.723 (0.018)	0.752 (0.016)	0.873 (0.010)
Slovakia	0.867 (0.012)	0.860 (0.012)	0.732 (0.017)	0.578 (0.024)	0.740 (0.017)
Italy	0.873 (0.012)	0.859 (0.012)	0.721 (0.017)	0.569 (0.023)	0.713 (0.018)
Moldova	0.933 (0.007)	0.909 (0.008)	0.783 (0.014)	0.703 (0.017)	0.843 (0.011)
Lithuania	0.781 (0.020)	0.798 (0.019)	0.623 (0.024)	0.480 (0.029)	0.643 (0.025)
Georgia	0.931 (0.007)	0.906 (0.008)	0.532 (0.025)	0.855 (0.011)	0.884 (0.010)
Bulgaria	0.794 (0.016)	0.836 (0.015)	0.737 (0.019)	0.607 (0.024)	0.720 (0.020)
Mongolia	0.815 (0.017)	0.781 (0.019)	0.675 (0.022)	0.634 (0.025)	0.741 (0.020)
Albania	0.870 (0.015)	0.785 (0.017)	0.624 (0.023)	0.604 (0.023)	0.713 (0.019)
Poland	0.834 (0.011)	0.866 (0.010)	0.696 (0.015)	0.706 (0.015)	0.746 (0.014)
Romania	0.809 (0.016)	0.863 (0.015)	0.624 (0.023)	0.534 (0.025)	0.612 (0.023)
Croatia	0.859 (0.012)	0.866 (0.012)	0.735 (0.017)	0.630 (0.022)	0.699 (0.019)

Note: All loadings are significant at $p<0.05$.

STANDARDIZED FACTOR LOADINGS Model 2 Trust in political and protective institutions

Country	Trust in Government	Trust in Parliament	Trust in Political Parties	Trust in Police	Trust in Armed Forces
Italy	0.845 (0.014)	0.884 (0.013)	0.728 (0.017)	0.891 (0.032)	0.887 (0.032)
Great Britain	0.831 (0.013)	0.866 (0.013)	0.694 (0.016)	0.835 (0.071)	0.415 (0.041)
Romania	0.769 (0.018)	0.907 (0.016)	0.615 (0.023)	0.887 (0.032)	0.787 (0.030)
Kosovo	0.767 (0.016)	0.919 (0.012)	0.768 (0.016)	0.872 (0.028)	0.816 (0.027)
Czech Rep	0.812 (0.015)	0.883 (0.014)	0.718 (0.019)	0.861 (0.034)	0.726 (0.031)
Croatia	0.844 (0.014)	0.882 (0.013)	0.737 (0.018)	0.827 (0.030)	0.782 (0.029)
Turkey	0.757 (0.021)	0.882 (0.020)	0.540 (0.026)	0.888 (0.034)	0.688 (0.031)
Slovakia	0.848 (0.014)	0.873 (0.013)	0.744 (0.017)	0.884 (0.028)	0.754 (0.027)
Bosnia	0.871 (0.010)	0.958 (0.008)	0.743 (0.015)	0.925 (0.020)	0.799 (0.020)
Latvia	0.749 (0.022)	0.849 (0.020)	0.613 (0.025)	0.647 (0.037)	0.729 (0.038)
Slovenia	0.786 (0.018)	0.871 (0.017)	0.629 (0.023)	0.820 (0.030)	0.726 (0.029)
Poland	0.805 (0.013)	0.888 (0.011)	0.713 (0.015)	0.831 (0.020)	0.802 (0.020)
Serbia	0.842 (0.011)	0.890 (0.010)	0.728 (0.014)	0.896 (0.021)	0.735 (0.020)
Lithuania	0.748 (0.021)	0.816 (0.020)	0.648 (0.024)	0.617 (0.038)	0.658 (0.040)
Germany	0.838 (0.015)	0.887 (0.014)	0.667 (0.020)	0.708 (0.036)	0.590 (0.034)
Mongolia	0.767 (0.021)	0.824 (0.020)	0.698 (0.022)	0.679 (0.035)	0.684 (0.035)
Estonia	0.760 (0.025)	0.802 (0.025)	0.479 (0.031)	0.666 (0.035)	0.693 (0.036)
France	0.767 (0.022)	0.780 (0.021)	0.577 (0.026)	0.802 (0.028)	0.729 (0.028)
Hungary	0.840 (0.013)	0.893 (0.012)	0.722 (0.018)	0.840 (0.019)	0.866 (0.019)
Sweden	0.782 (0.023)	0.802 (0.025)	0.615 (0.027)	0.528 (0.038)	0.730 (0.044)
Albania	0.797 (0.017)	0.832 (0.016)	0.681 (0.021)	0.653 (0.020)	0.741 (0.031)
Bulgaria	0.755 (0.018)	0.860 (0.015)	0.754 (0.018)	0.831 (0.025)	0.749 (0.025)
Georgia	0.930 (0.011)	0.905 (0.012)	0.538 (0.025)	0.852 (0.019)	0.820 (0.020)
Macedonia	0.811 (0.016)	0.834 (0.016)	0.682 (0.020)	0.853 (0.019)	0.812 (0.019)
Belarus	0.893 (0.012)	0.913 (0.012)	0.580 (0.026)	0.867 (0.029)	0.619 (0.029)
Ukraine	0.834 (0.011)	0.876 (0.010)	0.710 (0.015)	0.724 (0.021)	0.693 (0.021)
Moldova	0.906 (0.008)	0.931 (0.008)	0.796 (0.013)	0.746 (0.022)	0.780 (0.022)
Armenia	0.874 (0.011)	0.938 (0.009)	0.745 (0.017)	0.868 (0.030)	0.533 (0.029)
Kyrgyzstan	0.518 (0.029)	0.752 (0.024)	0.725 (0.024)	0.709 (0.025)	0.798 (0.025)
Montenegro	0.893 (0.010)	0.914 (0.009)	0.696 (0.018)	0.886 (0.015)	0.809 (0.017)
Russia	0.775 (0.014)	0.889 (0.012)	0.707 (0.017)	0.782 (0.019)	0.713 (0.019)
Tajikistan	0.786 (0.017)	0.858 (0.015)	0.672 (0.022)	0.827 (0.017)	0.841 (0.017)
Kazakhstan	0.876 (0.011)	0.901 (0.011)	0.730 (0.019)	0.803 (0.021)	0.735 (0.022)
Azerbaijan	0.806 (0.015)	0.901 (0.011)	0.758 (0.016)	0.908 (0.023)	0.562 (0.026)
Uzbekistan	0.905 (0.007)	0.936 (0.006)	0.731 (0.015)	0.805 (0.013)	0.886 (0.012)

Note: All loadings are significant at $p<0.05$.

STANDARDIZED FACTOR LOADINGS Model 3 Courts And Police Error Correlation

Country	Trust in Government	Trust in Parliament	Trust in Political Parties	Trust in Police	Trust in Courts
Kosovo	0.758 (.015)	0.903 (.010)	0.794 (.014)	0.385 (.028)	0.780 (.015)
Albania	0.758 (.017)	0.863 (.014)	0.686 (.020)	0.413 (.029)	0.693 (.020)
Georgia	0.895 (.009)	0.934 (.008)	0.561 (.025)	0.561 (.024)	0.768 (.016)
Mongolia	0.743 (.020)	0.817 (.018)	0.730 (.021)	0.404 (.032)	0.631 (.025)
Tajikistan	0.756 (.017)	0.880 (.012)	0.681 (.020)	0.646 (.023)	0.840 (.014)
Azerbaijan	0.786 (.014)	0.905 (.009)	0.776 (.015)	0.722 (.018)	0.863 (.011)
Macedonia	0.776 (.016)	0.866 (.013)	0.684 (.019)	0.571 (.024)	0.709 (.019)
Montenegro	0.877 (.009)	0.925 (.008)	0.705 (.018)	0.677 (.019)	0.832 (.012)
Estonia	0.733 (.023)	0.825 (.022)	0.494 (.030)	0.394 (.033)	0.537 (.030)
Italy	0.830 (.013)	0.890 (.012)	0.739 (.017)	0.290 (.030)	0.501 (.026)
Bulgaria	0.734 (.018)	0.859 (.015)	0.774 (.018)	0.544 (.026)	0.598 (.025)
France	0.727 (.021)	0.813 (.019)	0.588 (.025)	0.484 (.029)	0.565 (.026)
Croatia	0.827 (.013)	0.891 (.011)	0.747 (.017)	0.405 (.029)	0.661 (.020)
Latvia	0.718 (.022)	0.876 (.019)	0.615 (.025)	0.355 (.032)	0.463 (.031)
Britain	0.822 (.013)	0.873 (.012)	0.695 (.016)	0.309 (.026)	0.446 (.023)
Turkey	0.745 (.019)	0.885 (.016)	0.555 (.016)	0.448 (.029)	0.638 (.023)
Romania	0.762 (.016)	0.900 (.013)	0.635 (.022)	0.377 (.029)	0.708 (.019)
Sweden	0.762 (.024)	0.816 (.025)	0.619 (.027)	0.343 (.035)	0.442 (.033)
Belarus	0.880 (.012)	0.923 (.010)	0.588 (.026)	0.598 (.024)	0.651 (.022)
Armenia	0.867 (.010)	0.939 (.007)	0.754 (.017)	0.640 (.022)	0.851 (.012)
Serbia	0.834 (.011)	0.893 (.009)	0.734 (.014)	0.510 (.021)	0.664 (.017)
Kyrgyzstan	0.510 (.029)	0.782 (.023)	0.706 (.024)	0.526 (.030)	0.647 (.026)
Kazakhstan	0.868 (011)	0.893 (.010)	0.749 (.017)	0.641 (.022)	0.773 (.017)
Lithuania	0.743 (.021)	0.812 (.019)	0.656 (.024)	0.355 (.032)	0.528 (.028)
Czech Rep	0.800 (.015)	0.881 (.013)	0.736 (.018)	0.410 (.029)	0.590 (.024)
Poland	0.798 (.012)	0.889 (.010)	0.719 (.014)	0.466 (.022)	0.693 (.016)
Russia	0.756 (.014)	0.897 (.011)	0.719 (.016)	0.600 (.020)	0.671 (.018)
Slovakia	0.838 (.013	0.878 (.012)	0.751 (.017)	0.452 (.028)	0.635 (.022)
Moldova	0.897 (.008)	0.938 (.007)	0.798 (.013)	0.544 (.024)	0.738 (.016)
Hungary	0.831 (.013)	0.906 (.011)	0.712 (.018)	0.522 (.025)	0.636 (.021)
Ukraine	0.827 (.011)	0.880 (.010)	0.713 (.015)	0.526 (.021)	0.641 (.017)
Bosnia	0.865 (.009)	0.960 (.007)	0.748 (.015)	0.516 (0.024)	0.701 (.017)
Germany	0.833 (.014)	0.893 (.013)	0.664 (.020)	0.418 (.028)	0.561 (.024)
Uzbekistan	0.897 (.007)	0.945 (.006)	0.734 (.015)	0.649 (.017)	0.748 (.013)

STANDARDIZED FACTOR LOADINGS Simple Political Trust Model

Country	Trust in Government	Trust in Parliament	Trust in Political Parties	Trust in Local Government
Albania	0.800 (0.017)	0.852 (0.016)	0.646 (0.022)	0.611 (0.023)
Armenia	0.889 (0.010)	0.926 (0.009)	0.736 (0.018)	0.752 (0.016)
Azerbaijan	0.790 (0.014)	0.909 (0.009)	0.762 (0.016)	0.866 (0.011)
Belarus	0.907 (0.009)	0.898 (0.010)	0.559 (0.027)	0.853 (0.012)
Bosnia	0.887 (0.008)	0.943 (0.006)	0.741 (0.015)	0.856 (0.010)
Bulgaria	0.774 (0.017)	0.849 (0.015)	0.749 (0.018)	0.602 (0.024)
Croatia	0.844 (0.013)	0.879 (0.012)	0.740 (0.017)	0.626 (0.022)
Czech Rep	0.822 (0.016)	0.884 (0.014)	0.703 (0.019)	0.465 (0.027)
Estonia	0.689 (0.027)	0.882 (0.027)	0.476 (0.031)	0.382 (0.032)
France	0.735 (0.023)	0.813 (0.023)	0.576 (0.026)	0.399 (0.031)
Georgia	0.923 (0.008)	0.913 (0.008)	0.538 (0.025)	0.857 (0.011)
Germany	0.879 (0.012)	0.851 (0.013)	0.654 (0.020)	0.707 (0.018)
Great Britain	0.832 (0.012)	0.861 (0.011)	0.699 (0.016)	0.670 (0.017)
Hungary	0.851 (0.013)	0.903 (0.011)	0.685 (0.019)	0.695 (0.018)
Italy	0.848 (0.013)	0.880 (0.012)	0.730 (0.017)	0.569 (0.023)
Kazakhstan	0.907 (0.010)	0.884 (0.010)	0.697 (0.020)	0.809 (0.014)
Kosovo	0.796 (0.014)	0.898 (0.011)	0.763 (0.015)	0.754 (0.016)
Kyrgyzstan	0.622 (0.027)	0.748 (0.024)	0.635 (0.027)	0.708 (0.025)
Latvia	0.743 (0.022)	0.851 (0.020)	0.619 (0.025)	0.430 (0.031)
Lithuania	0.760 (0.021)	0.820 (0.020)	0.625 (0.025)	0.479 (0.029)
Macedonia	0.777 (0.016)	0.870 (0.013)	0.680 (0.019)	0.763 (0.016)
Moldova	0.917 (0.008)	0.927 (0.008)	0.785 (0.014)	0.704 (0.017)
Mongolia	0.782 (0.020)	0.814 (0.019)	0.684 (0.023)	0.646 (0.024)
Montenegro	0.934 (0.008)	0.880 (0.010)	0.669 (0.020)	0.817 (0.012)
Poland	0.812 (0.012)	0.887 (0.010)	0.701 (0.015)	0.702 (0.015)
Romania	0.782 (0.017)	0.892 (0.015)	0.620 (0.023)	0.526 (0.025)
Russia	0.759 (0.014)	0.915 (0.010)	0.689 (0.017)	0.773 (0.013)
Serbia	0.828 (0.011)	0.902 (0.009)	0.728 (0.014)	0.695 (0.015)
Slovakia	0.852 (0.013)	0.874 (0.013)	0.737 (0.017)	0.573 (0.024)
Sweden	0.773 (0.021)	0.823 (0.021)	0.608 (0.026)	0.691 (0.023)
Tajikistan	0.789 (0.015)	0.881 (0.012)	0.630 (0.022)	0.852 (0.013)
Turkey	0.779 (0.017)	0.854 (0.015)	0.543 (0.026)	0.778 (0.017)
Ukraine	0.844 (0.011)	0.873 (0.010)	0.699 (0.015)	0.632 (0.018)
Uzbekistan	0.913 (0.006)	0.934 (0.005)	0.726 (0.014)	0.883 (0.007)

Note: All loadings are significant at $p<0.05$.

3) Table of trust means per country

	Pres.	Gov't	Regional	Local	Parl.	Pol. Parties	Police	Armed Forces	Courts
Albania	3.009	2.595	2.733	2.806	2.535	2.419	3.249	3.219	2.325
Armenia	2.680	2.565	2.673	2.827	2.425	2.422	2.791	3.781	2.419
Azerbaijan	4.553	3.513	3.077	3.151	3.163	2.722	3.359	4.216	3.048
Belarus	3.430	3.339	3.282	3.234	3.390	2.784	3.261	3.555	3.415
Bosnia	2.236	2.193	2.243	2.363	2.232	2.128	2.982	2.907	2.437
Bulgaria	2.665	2.478	2.488	2.653	2.138	1.966	2.825	2.904	2.158
Croatia	3.213	1.904	2.255	2.267	1.880	1.807	3.118	3.313	2.211
Czech Rep	3.348	2.426	2.782	3.071	2.314	2.172	3.092	3.232	2.718
Estonia	3.628	2.899	3.363	3.404	2.748	2.573	3.840	3.958	3.283
France	2.365	2.381	3.048	3.406	2.652	2.038	3.308	3.444	2.782
Georgia	3.414	3.236	3.246	3.213	3.087	2.615	3.657	3.701	3.029
Germany	3.329	2.724	3.013	3.205	2.858	2.396	3.802	3.438	3.634
Gr. Britain	3.478	2.338	-	2.640	2.436	2.295	3.654	4.161	3.255
Hungary	3.177	2.945	-	3.302	2.777	2.271	3.043	2.985	3.049
Italy	3.053	2.235	2.566	2.693	2.305	1.954	3.702	3.631	2.728
Kazakhstan	4.072	3.515	3.416	3.355	3.455	3.063	3.033	3.507	3.046
Kosovo	2.594	2.410	-	2.790	2.547	2.496	2.818	3.788	2.525
Kyrgyzstan	2.412	2.461	2.728	2.876	2.556	2.538	2.724	3.303	2.234
Latvia	2.735	2.133	-	2.842	1.987	1.862	3.169	3.260	2.751
Lithuania	3.714	2.073	2.458	2.559	2.005	2.052	3.001	3.129	2.363
Macedonia	2.581	2.412	-	2.565	2.302	2.118	2.976	3.137	2.248
Moldova	2.342	2.366	2.499	2.633	2.287	2.276	2.528	3.026	2.282
Mongolia	2.899	2.817	2.987	3.077	2.725	2.346	3.319	3.724	2.655
Montenegro	3.268	3.128	-	3.123	3.053	2.793	3.282	3.374	3.139
Poland	3.079	2.798	3.027	3.080	2.731	2.506	3.348	3.461	3.090
Romania	1.803	1.594	2.216	2.514	1.643	1.594	2.855	3.139	1.998
Russia	3.432	3.116	2.865	2.794	2.736	2.411	2.648	3.244	2.677
Serbia	2.641	2.125	2.272	2.294	2.089	1.870	2.948	3.136	2.331
Slovakia	3.074	2.501	2.689	2.873	2.406	2.323	3.048	3.306	2.659
Slovenia	2.863	2.247	-	2.853	2.288	2.269	3.076	3.220	2.620
Sweden	3.441	3.536	3.265	3.379	3.598	3.085	4.013	3.344	3.985
Tajikistan	4.639	4.304	4.133	4.017	3.968	3.434	3.612	3.987	3.835
Turkey	3.595	3.395	3.491	3.452	3.334	2.994	3.697	3.799	3.429
Ukraine	2.504	2.367	2.492	2.653	2.205	2.212	2.313	2.991	2.116
Uzbekistan	-	4.401	4.337	4.283	4.369	4.017	3.918	4.343	4.023

Appendix B (Ch. 4)

Appendix Contents:

Table B1. Variable Descriptions

Variable Name	Description	Coding
High Corruption (dummy)	"In your opinion, how often do people like you have to make unofficial payments or gifts in these situations?"[1=Never, 5= Always] a) Interact with the road police, b) Request official documents (e.g. passport, visa, birth or marriage certificate, land register, etc.) from authorities, c) go to courts for a civil matter, d) Receive public education (primary or secondary), e) Receive public education (vocation), f) Receive medical treatment in the public health system, g) Request unemployment benefits, h) Request other social security benefits	All categories summed into one index, broken into terciles. "High corruption" dummy: 1 for respondents in the third tercile (those who perceive the highest amount of corruption), 0 for respondents in terciles 1-2.
Regime (Autocracy, Anocracy and Democracy, categorical predictor)	Polity Scores	1=Autocracy, 2= Anocracy, 3= Democracy
Satisfied with Financial Situation (categorical predictor)	"All things considered, I am satisfied with my job as a whole."[1= Strongly disagree, 5= Strongly agree]	5 response categories, those scoring '1' are reference
Satisfied with National Economy (categorical predictor)	"On the whole, I am satisfied with the present state of the economy." [1=strongly disagree, 5= strongly agree]	5 response categories, those scoring '1' are reference
Consumption Quartiles (categorical predictor)	"Approximately how much does your household spend on each of these items per month?" a) food, beverages and tobacco; b) utilities (electricity, water, gas, heating, fixed line phone); c) transportation (public transportation, fuel for car)	3 categories summed and converted to USD based on Sept 1, 2010 exchange rate. This measure is used to construct within-country consumption quartiles.
Ladder Question (categorical predictor)	"Please imagine a ten-step ladder where on the bottom, the first step, stand the poorest 10% people in our country, and on the highest step, the tenth, stand the richest 10% of people in our country. On which step of the ten is your household today? [1-10]	The ladder is collapsed into 5 categories: 1= steps 1&2, 2= steps 3&4, 3= step 5; 4= steps 6&7; 5= steps8-10

Life Satisfaction (categorical predictor)	"All things considered, I am satisfied with my life now" [1= strongly disagree, 5= strongly agree]	5 response categories, those scoring '1' are reference
Retrospective Improvement (categorical predictor)	I subtracted respondents' evaluations of their situation on the ladder four years ago from the step they believe they are on today (see Ladder Question). Positive numbers indicate that respondents believe there has been an improvement to their condition, negative numbers indicate a deterioration.	5 response categories. 1= a deterioration of 4 or more steps, 2= a deterioration of 1, 2 or 3 steps, 3= no change, 4= an improvement by 1, 2 or 3 steps; 5= an improvement by 4 or more steps
Prospective Improvement (categorical predictor)	I subtracted respondents' predictions of where they will be on the ladder 4 years from now from the step they believe they are on today (see Ladder Question). Positive numbers indicate there is an expected improvement in the future; negative indicate an expected deterioration.	5 response categories. 1= an expected deterioration of 4 or more steps, 2= an expected deterioration of 1, 2 or 3 steps, 3= no change, 4= an expected improvement by 1, 2 or 3 steps; 5= an expected improvement by 4 or more steps
Education (Low, Middle and High categorical predictor)	"what is the highest level of education you already completed?"	Three categories: 1= low ed (reference group, those with no ed, primary or lower secondary ed as highest attained), 2= middle ed (those with upper secondary ed as the highest attained), 3= high ed (those with a tertiary, BA or graduate level ed)

Table B2. Baseline OLS with country fixed effects (no interactions), DV= Political Trust sum score, scaled 1-100, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**
High Corruption	-2.953*** (0.334)	-2.462*** (0.325)	-3.640*** (0.373)	-3.504*** (0.341)
Economic Context (see column heading)				
2	8.295*** (0.460)	10.052*** (0.396)	-0.689 (0.471)	5.301*** (0.491)
3	14.143*** (0.471)	19.754*** (0.451)	-0.491 (0.488)	9.154*** (0.518)
4	19.671*** (0.494)	26.007*** (0.505)	-2.073*** (0.522)	13.336*** (0.570)
5	21.287*** (0.864)	25.173*** (0.923)	N/A	16.266*** (0.917)
Female	0.962*** (0.299)	0.466 (0.291)	0.884*** (0.333)	0.878*** (0.305)
Age	-0.276*** (0.048)	-0.234*** (0.047)	-0.342*** (0.054)	-0.354*** (0.049)
Age^2	0.003*** (0.000)	0.003*** (0.000)	0.004*** (0.001)	0.004*** (0.001)
Household Size	0.168 (0.103)	0.159 (0.099)	0.188 (0.122)	0.057 (0.105)
Urban	-1.142*** (0.324)	-1.086*** (0.315)	-0.937*** (0.362)	-1.334*** (0.330)
Capital	-1.975*** (0.399)	-1.820*** (0.388)	-1.699*** (0.451)	-2.130*** (0.407)
Middle Ed	0.246 (0.392)	1.187*** (0.379)	1.220*** (0.437)	0.212 (0.400)
Upper Ed	0.114 (0.412)	1.689*** (0.397)	2.075*** (0.460)	0.281 (0.423)
Anocracy	-11.022*** (1.284)	-9.996*** (1.240)	-12.065*** (1.471)	-11.435*** (1.305)
Democracy	-44.001*** (1.131)	-41.394*** (1.105)	-44.595*** (1.269)	-46.945*** (1.159)

R Sq	0.323	0.359	0.270	0.289
N	23313	23253	20383	23538

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p<0.01

Table B3. OLS with country fixed effects, DV= Political trust sum score, scaled 1-100, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**
High Corruption	-6.047*** (1.402)	-5.026*** (1.377)	-7.054*** (1.443)	-8.441*** (1.489)
Economic Context (see column heading)				
2	8.875*** (0.559)	10.183*** (0.484)	-0.396 (0.568)	4.995*** (0.588)
3	14.441*** (0.574)	20.289*** (0.545)	-0.276 (0.588)	8.839*** (0.622)
4	19.006*** (0.598)	25.543*** (0.613)	-2.000*** (0.627)	13.296*** (0.688)
5	21.586*** (1.030)	25.761*** (1.121)	N/A	15.365*** (1.148)
Ref= Corr x 1	Corr x Sat. with financial situation	Corr x Sat. with national economy	Corr x consumption quartiles	Corr x ladder
Corr x 2	**-1.836* (0.970)**	**-0.373 (0.822)**	**-1.009 (0.989)**	**0.943 (1.056)**
Corr x 3	**-0.966 (0.975)**	**-1.630* (0.918)**	**-0.776 (0.986)**	**0.928 (1.094)**
Corr x 4	**1.821* (1.014)**	**1.172 (0.996)**	**-0.409 (1.000)**	**0.112 (1.176)**
Corr x 5	**-1.238 (1.851)**	**-1.880 (1.911)**	**N/A**	**2.419 (1.865)**
Female	0.963*** (0.299)	0.474 (0.291)	0.885*** (0.333)	0.881*** (0.305)
Age	-0.278*** (0.048)	-0.236*** (0.047)	-0.343*** (0.542)	-0.356*** (0.049)
Age^2	0.003*** (0.000)	0.003*** (0.000)	0.004*** (0.001)	0.004*** (0.001)
Household Size	0.174* (0.104)	0.161 (0.099)	0.199 (0.122)	0.058 (0.105)
Urban	-1.165*** (0.323)	-1.112*** (0.315)	-0.963*** (0.362)	-1.369*** (0.330)
Capital	-1.941*** (0.399)	-1.779*** (0.388)	-1.653*** (0.451)	-2.090*** (0.408)
Middle Ed	0.270 (0.466)	1.299*** (0.452)	1.162** (0.522)	0.248 (0.476)
Upper Ed	0.585 (0.484)	2.063*** (0.467)	2.164*** (0.544)	0.672 (0.496)
Mid. Ed x Corr	-0.155 (0.793)	-0.439 (0.769)	0.125 (0.884)	-0.188 (0.812)
Upper Ed x Corr	-1.309* (0.784)	-1.107 (0.760)	-0.180 (0.878)	-1.068 (0.803)
Anocracy	-12.707*** (1.369)	-11.758*** (1.329)	-14.113*** (1.581)	-13.237*** (1.391)
Democracy	-45.395*** (1.175)	-42.514*** (1.155)	-45.693*** (1.316)	-48.317*** (1.201)
Corr x Anoc	**4.873*** (1.305)**	**4.813*** (1.290)**	**5.801*** (1.495)**	**5.868*** (1.335)**
Corr x Democ	**4.407*** (1.118)**	**3.608*** (1.121)**	**4.145*** (1.262)**	**5.035*** (1.146)**
R Sq	0.324	0.361	0.271	0.289
N	23313	23253	20383	23538

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p<0.01

Table B4. DV is sum score without local government, scaled 1-100, country fixed effects, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**
High Corruption	-5.833*** (1.439)	-4.816*** (1.409)	-6.687*** (1.482)	-8.408*** (1.524)
Economic Context (see column heading)				
2	8.848*** (0.573)	10.036*** (0.494)	-0.238 (0.581)	4.779*** (0.601)
3	14.689*** (0.588)	21.046*** (0.556)	-0.002 (0.602)	8.525*** (0.636)
4	19.162*** (0.612)	26.191*** (0.626)	-1.665*** (0.642)	13.412*** (0.704)
5	21.548*** (1.051)	26.351*** (1.138)	N/A	15.679*** (1.177)
Ref= Corr x 1	Corr x Sat. with financial situation	Corr x Sat. with national economy	Corr x consumption quartiles	Corr x ladder
Corr x 2	**-1.951** (0.994)**	**-0.247 (0.839)**	**-1.242 (1.013)**	**1.159 (1.078)**
Corr x 3	**-1.132 (0.999)**	**-1.686* (0.938)**	**-1.228 (1.010)**	**0.968 (1.117)**
Corr x 4	**1.401 (1.038)**	**1.093 (1.017)**	**-0.871 (1.025)**	**-0.197 (1.202)**
Corr x 5	**-0.105 (1.892)**	**-1.738 (1.947)**	**N/A**	**2.836 (1.908)**
Female	0.901*** (0.306)	0.364 (0.297)	0.752** (0.341)	0.808*** (0.312)
Age	-0.284*** (0.049)	-0.240*** (0.048)	-0.361*** (0.055)	-0.363*** (0.049)
Age^2	0.003*** (0.001)	0.003*** (0.000)	0.004*** (0.001)	0.004*** (0.001)
Household Size	0.132 (0.105)	0.119 (0.102)	0.121 (0.125)	0.012 (0.107)
Urban	-0.741** (0.332)	-0.658** (0.322)	-0.441 (0.371)	-0.958*** (0.338)
Capital	-1.621*** (0.408)	-1.391*** (0.396)	-1.239*** (0.462)	-1.755*** (0.417)
Middle Ed	0.112 (0.478)	1.122** (0.461)	1.024* (0.535)	0.113 (0.488)
Upper Ed	0.718 (0.495)	2.154*** (0.476)	2.296*** (0.556)	0.793 (0.507)
Mid. Ed x Corr	-0.095 (0.812)	-0.420 (0.786)	0.032 (0.906)	-0.202 (0.831)
Upper Ed x Corr	-1.131 (0.803)	-1.003 (0.776)	-0.121 (0.899)	-0.936 (0.822)
Anocracy	-13.312*** (1.406)	-12.382*** (1.361)	-14.513*** (1.625)	-13.907*** (1.427)
Democracy	-47.245*** (1.209)	-44.305*** (1.184)	-48.018*** (1.354)	-50.283*** (1.234)
Corr x Anoc	**4.276*** (1.341)**	**4.212*** (1.323)**	**5.543*** (1.536)**	**5.369*** (1.371)**
Corr x Democ	**4.409*** (1.149)**	**3.513*** (1.148)**	**4.126*** (1.297)**	**5.093*** (1.176)**
R Sq	0.331	0.371	0.282	0.299
N	23581	23521	20608	23813

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01

Table B5. DV= Presidential Trust scaled 1-100, OLS with country fixed effects, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**
High Corruption	-1.696 (1.516)	-0.248 (1.428)	-2.952** (1.469)	-1.558 (1.608)
Economic Context (see column heading)				
2	10.942*** (1.171)	12.871*** (1.139)	1.203 (1.251)	5.741*** (1.149)
3	16.648*** (1.234)	22.473*** (1.261)	-0.082 (1.316)	11.903*** (1.279)
4	22.656*** (1.339)	29.604*** (1.384)	-0.485 (1.398)	13.963*** (1.528)
5	23.618*** (2.723)	30.030*** (3.043)	N/A	14.718*** (3.363)
Ref= Corr x 1	Corr x Sat. with financial situation	Corr x Sat. with national economy	Corr x consumption quartiles	Corr x ladder
Corr x 2	**-1.909 (1.878)**	**-1.352 (1.777)**	**-2.220 (1.976)**	**-1.892 (1.866)**
Corr x 3	**-1.181 (1.941)**	**-3.032 (1.915)**	**0.810 (1.985)**	**-4.121** (2.013)**
Corr x 4	**-4.243** (2.053)**	**-4.321** (2.012)**	**0.549 (2.012)**	**-3.430 (2.301)**
Corr x 5	**-9.078** (4.263)**	**-12.379*** (4.301)**	**N/A**	**-2.271 (4.728)**
Female	1.452** (0.650)	0.966 (0.643)	0.928 (0.720)	1.142* (0.659)
Age	-0.113 (0.101)	-0.109 (0.099)	-0.256** (0.111)	-0.203** (0.101)
Age^2	0.003*** (0.001)	0.003** (0.001)	0.004*** (0.001)	0.004*** (0.001)
Household Size	0.177 (0.219)	0.198 (0.215)	0.164 (0.255)	0.006 (0.221)
Urban	0.467 (0.703)	0.541 (0.698)	0.358 (0.788)	0.244 (0.713)
Capital	-1.544* (0.930)	-0.958 (0.917)	-0.296 (1.045)	-1.372 (0.938)
Middle Ed	1.759* (0.968)	2.448*** (0.954)	2.442** (1.067)	1.623* (0.979)
Upper Ed	-0.487 (0.941)	0.713 (0.928)	0.735 (1.045)	-0.499 (0.956)
Anocracy	-30.384*** (1.568)	-29.715*** (1.549)	-34.878*** (1.754)	-32.839*** (1.573)
Democracy	-37.483*** (1.337)	-33.425*** (1.344)	-41.377*** (1.443)	-39.099*** (1.357)
R Sq	0.415	0.438	0.391	0.392
N	8048	7931	6955	8140

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01

Table B6. Weighted Fixed Effects OLS, DV= Political Trust sum score, scaled 1-100, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**
High Corruption	-1.911 (2.730)	-3.772 (2.672)	-7.843*** (2.643)	-7.778*** (2.987)
Economic Context (see column heading)				
2	12.321*** (1.435)	11.317*** (1.059)	-2.082* (1.249)	4.943*** (1.606)
3	17.360*** (1.494)	22.739*** (1.513)	0.381 (1.442	10.805*** (1.691)
4	23.389*** (1.568)	28.564*** (1.564)	-2.538 (1.584)	15.088*** (1.929)
5	23.544*** (2.253)	28.413*** (3.310)	N/A	17.295*** (2.529)
Ref= Corr x 1	Corr x Sat. with financial situation	Corr x Sat. with national economy	Corr x consumption quartiles	Corr x ladder
Corr x 2	**-6.010*** (2.239)**	**-0.873 (1.810)**	**-0.288 (1.944)**	**1.259 (2.407)**
Corr x 3	**-4.272* (2.292)**	**-2.962 (2.033)**	**-0.161 (1.869)**	**0.039 (2.298)**
Corr x 4	**-2.324 (2.392)**	**-0.975 (2.239)**	**2.664 (2.132)**	**0.671 (2.631)**
Corr x 5	**-0.291 (3.772)**	**-0.289 (3.923)**	**N/A**	**7.202* (4.011)**
Female	1.815*** (0.665)	1.211** (0.599)	1.522* (0.803)	1.597** (0.688)
Age	-0.467*** (0.102)	-0.329*** (0.099)	-0.379*** (0.107)	-0.474*** (0.101)
Age^2	0.006*** (0.001)	0.005*** (0.001)	0.005*** (0.001)	0.006*** (0.01)
Household Size	0.236 (0.249)	0.245 (0.237)	0.389 (0.284)	0.260 (0.256)
Urban	-1.733 (1.199)	-1.771 (1.082)	-1.579 (1.263)	-1.301 (1.257)
Capital	-2.091 (1.936)	-2.503 (1.826)	-1.842 (2.156)	-2.093 (2.004)
Middle Ed	-0.224 (1.098)	0.791 (1.019)	0.392 (1.209)	-0.482 (1.113)
Upper Ed	-0.763 (1.185)	0.448 (1.084)	0.450 (1.238)	-0.859 (1.241)
Mid. Ed x Corr	-0.446 (1.808)	-0.377 (1.711)	-0.388 (1.925)	-1.473 (1.726)
Upper Ed x Corr	-2.559 (1.693)	-1.179 (1.562)	-1.686 (1.671)	-2.573 (1.739)
Anocracy	-14.521*** (2.722)	-13.243*** (2.579)	-15.837*** (3.273)	-15.020*** (2.848)
Democracy	-45.364*** (2.644)	-42.396** (2.075)	-46.209*** (2.703)	-49.559*** (2.529)
Corr x Anoc	**9.412*** (2.919)**	**8.902*** (2.728)**	**10.001*** (3.435)**	**10.379*** (2.916)**
Corr x Democ	**5.415*** (2.098)**	**4.609** (2.075)**	**6.830*** (2.321)**	**7.220*** (2.143)**
R Sq	0.317	0.359	0.270	0.283
N	23357	23297	20427	23582

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01

Table B7. SEM with DV as a latent factor of political trust, no fixed effects, ML estimation, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**
High Corruption	-0.479*** (0.058)	-0.482*** (0.056)	-0.615*** (0.058)	-0.582*** (0.060)
Economic Context (see column heading)				
2	0.447*** (0.025)	0.496*** (0.021)	-0.073*** (0.026)	0.249*** (0.026)
3	0.674*** (0.025)	0.974*** (0.024)	-0.073*** (0.026)	0.429*** (0.028)
4	0.975*** (0.026)	1.354*** (0.026)	-0.181*** (0.028)	0.599*** (0.030)
5	1.118*** (0.026)	1.370*** (0.049)	N/A	0.711*** (0.052)
	Corr x Sat. with	Corr x Sat. with	Corr x consumption	Corr x ladder
Ref= Corr x 1	financial situation	national economy	quartiles	
Corr x 2	**-0.123*** (0.044)**	**-0.012 (0.037)**	**-0.032 (0.045)**	**0.007 (0.047)**
Corr x 3	**-0.073* (0.044)**	**-0.048 (0.041)**	**-0.017 (0.045)**	**0.003 (0.049)**
Corr x 4	**0.011 (0.045)**	**0.024 (0.044)**	**-0.011 (0.045)**	**0.006 (0.053)**
Corr x 5	**-0.036 (0.083)**	**0.028 (0.084)**	**N/A**	**0.116 (0.085)**
Female	0.064*** (0.013)	0.029** (0.013)	0.053*** (0.015)	0.059*** (0.014)
Age	-0.016*** (0.002)	-0.012*** (0.002)	-0.018*** (0.002)	-0.020*** (0.002)
Age^2	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)
Household Size	0.054*** (0.004)	0.042*** (0.004)	0.064*** (0.005)	0.055*** (0.004)
Urban	-0.095*** (0.014)	-0.085*** (0.014)	-0.076*** (0.016)	-0.114*** (0.014)
Capital	-0.078*** (0.017)	-0.079*** (0.017)	-0.057*** (0.020)	-0.081*** (0.018)
Middle Ed	0.044** (0.021)	0.093*** (0.020)	0.083*** (0.023)	0.048** (0.021)
Upper Ed	0.055*** (0.020)	0.099*** (0.020)	0.115*** (0.023)	0.071*** (0.021)
Mid. Ed x Corr	-0.028 (0.036)	-0.027 (0.034)	-0.014 (0.040)	-0.029 (0.037)
Upper Ed x Corr	-0.084** (0.035)	-0.069** (0.034)	-0.054 (0.040)	-0.077** (0.036)
Anocracy	-1.027*** (0.034)	-0.838*** (0.033)	-1.212*** (0.039)	-1.115*** (0.034)
Democracy	-1.296*** (0.028)	-1.014*** (0.028)	-1.429*** (0.032)	-1.396*** (0.028)
Corr x Anoc	**0.452*** (0.051)**	**0.501*** (0.050)**	**0.584*** (0.057)**	**0.507*** (0.052)**
Corr x Democ	**0.453*** (0.043)**	**0.409*** (0.043)**	**0.516*** (0.048)**	**0.470*** (0.470)**
Goodness of Fit	Chi Sq=1111.571 (DF=71), RMSEA=0.024, CFI=0.983, SRMR=0.007	Chi Sq= 1215.794 (DF=71), RMSEA=0.025, CFI=0.982, SRMR=0.007	Chi Sq= 920.802 (DF=65), RMSEA=0.024, CFI=0.983, SRMR=0.008	Chi Sq= 1077.398 (DF=71), RMSEA=0.023, CFI=0.983, SRMR=0.007
N	25509	25368	22188	25805

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01

Table B8. 2 Level Random Slope, ML Estimation (no country dummies), DV= Political Trust scaled 1-100, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**
High Corruption	-5.222*** (1.935)	-4.148** (1.936)	-6.259*** (2.071)	-7.663*** (2.097)
Economic Context (see column heading)				
2	8.975*** (0.560)	10.266*** (0.486)	-0.392 (0.567)	5.063*** (0.588)
3	14.607*** (0.576)	20.436*** (0.549)	-0.264 (0.587)	8.963*** (0.624)
4	19.279*** (0.603)	25.756*** (0.622)	-1.941 (0.627)	13.455*** (0.692)
5	21.851*** (1.033)	25.963*** (1.126)	N/A	15.582*** (1.152)
Ref= Corr x 1	Corr x Sat. with financial situation	Corr x Sat. with national economy	Corr x consumption quartiles	Corr x ladder
Corr x 2	**-2.126** (0.975)**	**-0.558 (0.832)**	**-0.924 (0.989)**	**0.816 (1.058)**
Corr x 3	**-1.447 (0.989)**	**-1.966** (0.944)**	**-0.653 (0.985)**	**0.733 (1.104)**
Corr x 4	**1.011 (1.036)**	**0.667 (1.037)**	**-0.292 (1.001)**	**-0.391 (1.197)**
Corr x 5	**-2.036 (1.866)**	**-2.299 (1.941)**	**N/A**	**1.809 (1.888)**
Female	0.974*** (0.299)	0.493* (0.290)	0.901*** (0.333)	0.893*** (0.305)
Age	-0.275*** (0.048)	-0.234*** (0.047)	-0.343*** (0.054)	-0.355*** (0.049)
Age^2	0.003*** (0.000)	0.003*** (0.000)	0.004*** (0.001)	0.004*** (0.105)
Household Size	0.184** (0.102)	0.169* (0.099)	0.199 (0.121)	0.069 (0.105)
Urban	-1.197*** (0.323)	-1.149*** (0.315)	-0.997*** (0.362)	-1.393*** (0.330)
Capital	-1.931*** (0.400)	-1.804*** (0.389)	-1.609*** (0.453)	-2.071*** (0.409)
Middle Ed	0.307 (0.468)	1.348*** (0.453)	1.159** (0.524)	0.224 (0.479)
Upper Ed	0.692 (0.490)	2.226*** (0.473)	2.292*** (0.551)	0.749 (0.504)
Mid. Ed x Corr	-0.354 (0.809)	-0.656 (0.785)	0.078 (0.904)	-0.186 (0.829)
Upper Ed x Corr	-1.729** (0.828)	-1.693** (0.801)	-0.704 (0.925)	-1.393 (0.852)
Anocracy	-18.695*** (6.697)	-14.701** (5.831)	-19.798*** (7.569)	-19.716*** (7.329)
Democracy	-25.401*** (5.448)	-19.616*** (4.747)	-26.157*** (6.157)	-27.167*** (5.963)
Corr x Anoc	**5.013** (2.187)**	**4.814** (2.199)**	**5.315** (2.468)**	**5.527** (2.374)**
Corr x Democ	**4.323** (1.822)**	**3.249* (1.841)**	**3.644* (2.047)**	**4.704** (1.974)**
ICC	.16	.13	.19	.19
N	23313	23253	20383	23538

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01. In this two-level analysis, a variance components analysis indicates that there is significant variation in the trust-corruption relationship across countries, with an ICC of 0.28. The ICC listed in this table indicates the correlation of political trust responses between two people after accounting for regime type. Each ICC drops by at least 0.09.

Table B9. Alternative predictors (DV=political trust 1-100), fixed effects OLS, 30 countries

	Life Satisfaction	**Retrospective Improvement**	**Prospective Improvement**
High Corruption	-7.769*** (1.565)	-7.821*** (2.445)	-3.138 (4.571)
Economic Context (see column heading)			
2	8.132*** (0.726)	6.253*** (1.270)	6.226** (2.698)
3	14.479*** (0.706)	9.175*** (1.271)	10.809*** (2.680)
4	19.713*** (0.695)	10.851*** (1.307)	13.784*** (2.684)
5	22.000*** (0.968)	13.019*** (3.159)	10.591*** (2.914)
	Corr x Life	Corr x Retrospective	Corr x Prospective
Ref= Corr x 1	Satisfaction	Evaluations	Evaluations
Corr x 2	**-0.180 (1.231)**	**0.061 (2.175)**	**-4.711 (4.442)**
Corr x 3	**0.566 (1.192)**	**1.325 (2.165)**	**-3.768 (4.405)**
Corr x 4	**1.549 (1.169)**	**-0.434 (2.216)**	**-4.537 (4.406)**
Corr x 5	**1.819 (1.636)**	**-5.716 (4.651)**	**-5.941 (4.746)**
Female	0.740** (0.298)	0.897*** (0.309)	1.077*** (0.329)
Age	-0.256*** (0.048)	-0.347*** (0.049)	-0.294*** (0.053)
Age^2	0.003*** (0.000)	0.004*** (0.001)	0.003*** (0.001)
Household Size	0.039 (0.102)	0.129 (0.106)	0.088 (0.114)
Urban	-1.250*** (0.323)	-0.971*** (0.336)	-1.026*** (0.356)
Capital	-1.689*** (0.398)	-2.022*** (0.414)	-1.904*** (0.442)
Middle Ed	-2.310* (1.216)	1.089** (0.482)	1.521*** (0.512)
Upper Ed	-2.577** (1.138)	2.345*** (0.498)	3.062*** (0.529)
Mid. Ed x Corr	-0.299 (0.792)	-0.066 (0.818)	-0.269 (0.869)
Upper Ed x Corr	-1.156 (0.783)	-1.087 (0.808)	-0.884 (0.862)
Anocracy	-12.519*** (1.363)	-12.787*** (1.410)	-12.856*** (1.501)
Democracy	-44.170*** (1.176)	-46.317*** (1.218)	-45.629*** (1.283)
Corr x Anoc	**5.398*** (1.307)**	**6.207*** (1.355)**	**6.988*** (1.461)**
Corr x Democ	**5.088*** (1.124)**	**4.953*** (1.168)**	**4.919*** (1.254)**
R Sq	0.319	0.275	0.282
N	23552	23371	20559

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01

Table B10. 3 Level Analysis (contextual effects), DV=political trust 1-100, 30 countries

	Model 1: Regional Proportion Most Satisfied with Personal Finances	**Model 2: Regional Proportion Most Satisfied with National Economy**	**Model 3: Regional Proportion on Steps 6-10 of the Ladder**
High Corruption	-6.282*** (2.121)	-3.788* (2.213)	-6.848*** (1.982)
Economic Context (see column heading)			
2	7.776*** (0.452)	9.489*** (0.393)	4.759*** (0.481)
3	13.241*** (0.465)	18.618*** (0.451)	8.576*** (0.511)
4	18.754*** (0.492)	24.760*** (0.507)	12.206*** (0.569)
5	20.391*** (0.851)	24.029*** (0.916)	15.462*** (0.909)
Regional Proportion	4.201 (3.747)	9.932** (3.947)	4.142 (3.494)
Corr x Regional Proportion	**2.193 (3.631)**	**-2.039 (3.628)**	**1.714 (3.672)**
Female	1.075*** (0.289)	0.647** (0.282)	0.955*** (0.295)
Age	-0.259*** (0.047)	-0.223*** (0.046)	-0.339*** (0.047)
Age^2	0.003*** (0.000)	0.003*** (0.000)	0.004*** (0.000)
Household Size	0.109 (0.100)	0.108 (0.098)	-0.016 (0.102)
Urban	-1.351*** (0.348)	-1.203*** (0.339)	-1.613*** (0.355)
Capital	-4.681*** (1.093)	-4.429*** (1.024)	-5.175*** (1.123)
Middle Ed	-0.296 (0.456)	0.671 (0.444)	-0.317 (0.466)
Upper Ed	0.308 (0.478)	1.747*** (0.463)	0.536 (0.489)
Mid. Ed x Corr	-0.623 (0.794)	-0.879 (0.775)	-0.527 (0.809)
Upper Ed x Corr	-1.834** (0.807)	-1.771** (0.788)	-1.703** (0.824)
Anocracy	-18.698*** (6.192)	-13.313*** (5.020)	-19.901*** (7.010)
Democracy	-24.722*** (5.005)	-17.463*** (4.117)	-27.134*** (5.655)
Corr x Anoc	**5.405** (2.239)**	**4.518** (2.275)**	**5.792** (2.342)**
Corr x Democ	**3.796** (1.765)**	**2.599 (1.920)**	**4.072** (1.798)**
ICC (at region)	0.23	0.18	0.26
N	23313	23253	23538

Note: In this three-level analysis, there are 30 countries and 444 subnational regions with an average of 52.5 observations per region. The 'regional proportion' variable calculates the proportion of individuals in each region who scored a 4 or 5 (out of 5) on satisfaction with personal finances or the national economy, or steps 6-10 (out of 10) on the income ladder. A variance components analysis indicates that there is significant variation in the trust-corruption relationship on both country and regional levels. The intra-class correlation (ICC) of political trust responses at the region level is .34 (or the correlation of responses between two people in the same region). The regional ICC in this table indicates the correlation between two people after accounting for region-level proportions of economically satisfied strata.

Table B11. ZZC Replication (in their sample)

	Government Bribe Victimization	Bribe Victimization	High Bribe Victimization
Corruption	-5.67 (3.51)	5.78*** (1.87)	2.26 (3.33)
Sociotropic (Ref 1)			
2	7.03*** (0.66)	8.89*** (0.90)	7.52*** (0.66)
3	14.45*** (0.65)	17.68*** (0.89)	15.07*** (0.66)
4	20.93*** (0.74)	24.45*** (1.00)	21.46*** (0.74)
5	21.96*** (1.53)	28.19*** (2.07)	22.07*** (1.53)
Pocketbook (Ref 1)			
2	1.12 (0.93)	0.99 (1.38)	1.09 (0.94)
3	2.15** (0.91)	2.40* (1.31)	2.03** (0.91)
4	5.69*** (0.97)	5.51*** (1.41)	5.57*** (0.97)
5	6.29*** (1.43)	5.87*** (1.87)	6.23*** (1.39)
Corr x Sociotropic			
Corr x 2	2.42 (2.33)	**-3.56*** (1.20)**	-4.07** (2.02)
Corr x 3	3.91 (2.41)	**-6.02*** (1.18)**	-4.72** (1.90)
Corr x 4	3.49 (2.47)	**-6.81*** (1.35)**	-4.04* (2.20)
Corr x 5	0.07 (5.78)	**-12.37*** (2.74)**	0.21 (5.05)
Corr x Pocketbook			
Corr x 2	0.21 (3.66)	0.06 (1.87)	0.79 (3.39)
Corr x 3	0.58 (3.50)	-0.54 (1.79)	2.63 (3.19)
Corr x 4	0.71 (3.62)	0.44 (1.89)	3.28 (3.34)
Corr x 5	4.79 (5.30)	0.91 (2.61)	8.37 (5.80)
Voted for Incumb	12.61*** (0.35)	12.55*** (0.35)	12.58*** (0.35)
Income	-4.52*** (0.80)	-4.79*** (0.79)	-4.72*** (0.80)
Age	-0.42*** (0.09)	-0.39*** (0.09)	-0.41*** (0.09)
Female	0.99*** (0.26)	1.07*** (0.26)	1.04*** (0.25)
Educ	-3.15*** (0.67)	-3.17*** (0.66)	-3.32*** (0.67)
Urban	-0.67 (0.44)	-0.72 (0.44)	-0.72 (0.45)
R sq	0.28	0.28	0.28
N	28464	28594	28594

Note: Standard errors in parentheses. *p< 0.1, **p<0.05, ***p<0.01. Each column represents a replication of the ZZC model with a different petty corruption perceptions measure.) *Government bribe victimization* = "In the last twelve months, did any government employee ask you for a bribe?", *local bribe victimization* = asked to pay a bribe either at school, hospital or work in the last 12 months, *high local bribe victimization* = asked to pay a bribe in all three of the above places (school, hospital and work) in the last 12 months.

18 Table B12. Predicted Probabilities of Critical Authority Orientations Among High and Low Corruption Perceivers Across Levels Economic Satisfaction

Sat. with National Economy	1	2	3	4	5
Low/No Corruption	.448	.353	.242	.269	.204
95% CI	*.421-.474*	*.331-.376*	*.215-.268*	*.242-.298*	*.119-.289*
High Corruption	.443	.323	.232	.249	.225
95% CI	*.406-.479*	*.286-.361*	*.195-.269*	*.203-.294*	*.163-.287*
Sat. with Personal Finances	1	2	3	4	5
Low/No Corruption	.432	.335	.281	.309	.254
95% CI	*.401-.462*	*.312-.358*	*.259-.303*	*.282-.336*	*.179-.328*
High Corruption	.437	.332	.263	.284	.287
95% CI	*.399-.475*	*.299-.365*	*.217-.309*	*.237-.331*	*.228-.346*

Question Wording:

Authority orientations are measured by the following question: "How would you place your views on this scale? 1 means you agree completely with the statement on the left; 10 means you agree completely with the statement on the right, and if your views fall somewhere in between, you can choose any number in between, you can choose any number in between.

"As citizens, we should be more active in questioning the actions of our authorities" (1) → "In our country today, we should show more respect for our authorities" (10)

The binary category in this probit estimation was composed of respondents who answered '1' on this scale versus the rest of the scale. The analysis was repeated for relative critical authority orientations by combining the top three Likert response scale options into a binary category against the rest (54.91 percent of the sample). The results are robust to this analysis. Replication files are available upon request from the author.

Note on the Analysis:

These predicted probabilities are calculated holding all predictors at their means, medians or modes (respondents who are female, of average age and education, in average household size and urbanity). Standard errors are clustered at the country level.

19 Table B13. Predicted Probabilities of Preferences for Democracy Among High and Low Corruption Perceivers Across Levels of Economic Satisfaction

Sat. with National Economy	1	2	3	4	5
Low/No Corruption	.514	.546	.569	.591	.618
95% CI	*.475-.553*	*.522-.570*	*.544-.596*	*.555-.626*	*.571-.665*
High Corruption	.546	.546	.528	.552	.501
95% CI	*.508-.585*	*.515-.576*	*.475-.580*	*.507-.597*	*.404-.597*
Sat. with Personal Finances	1	2	3	4	5
Low/No Corruption	.475	.544	.558	.606	.639
95% CI	*.440-.509*	*.519-.569*	*.535-.581*	*.583-.629*	*.594-.684*
High Corruption	.499	.534	.529	.586	.589
95% CI	*.455-.545*	*.502-.567*	*.492-.565*	*.538-.634*	*.526-.654*

Question Wording:

Preferences for Democracy are measured by the following question:

"With which one of the following statements do you agree most?"

- "Democracy is preferable to any other form of political system"
- "Under some circumstances, an authoritarian government may be preferable to a democratic one"
- "For people like me, it does not matter whether a government is democratic or authoritarian"

The binary category in this probit estimation was composed of respondents who answered that democracy is preferable (54.39 percent of the sample) versus those who chose the other two options.

Note on the Analysis:

These predicted probabilities are calculated holding all predictors at their means, medians or modes (respondents who are female, of average age and education, in average household size and urbanity). Standard errors are clustered at the country level.

20 Table B14. Predicted Probabilities of Election Preferences Among High and Low Corruption Perceivers Across Levels of Satisfaction with National Economy

	1	2	3	4	5
Local Elections					
Low/No Corruption	.779	.778	.765	.777	.738
95% CI	*.752-.808*	*.759-.796*	*.741-.790*	*.739-.815*	*.681-.796*
High Corruption	.748	.721	.708	.742	.685
95% CI	*.719-.777*	*.699-.744*	*.683-.733*	*.715-.769*	*.599-.771*
	1	2	3	4	5
Regional Elections					
Low/No Corruption	.687	.665	.641	.643	.613
95% CI	*.654-.719*	*.638-.692*	*.604-.679*	*.591-.696*	*.562-.664*
High Corruption	.640	.628	.572	.590	.492
95% CI	*.604-.676*	*.595-.660*	*.533-611*	*.547-.634*	*.363-.621*

Question Wording:

Election Preferences are measured by the following question:

"Do you think elections of the leaders of local (and regional) administration are necessary, or should these leaders be appointed by higher authorities?"

- "elections are necessary"
- "leaders should be appointed"
- "it does not matter"

The binary category in this probit estimation was composed of respondents who answered that elections are necessary (77.19 percent of the sample prefer local elections; 66.90 percent of the sample prefer regional elections) versus those who chose the other two options at both levels of government.

Note on the Analysis:

These predicted probabilities are calculated holding all predictors at their means, medians or modes (respondents who are female, of average age and education, in average household size and urbanity). Standard errors are clustered at the country level. Results are robust to pocketbook economic satisfaction. Some interpretive caution is required because regional and local government levels can mean different things to respondents across these countries.

21 Table B15. Predicted Probabilities of Corruption Condemnation Among High and Low Corruption Perceivers Across Levels of Economic Satisfaction

Public Official Favor	1	2	3	4	5
Low/No Corruption	.687	.665	.641	.643	.613
95% CI	*.654-.719*	*.638-.692*	*.604-.679*	*.591-.696*	*.562-.664*
High Corruption	.640	.628	.572	.590	.492
95% CI	*.604-.676*	*.595-.660*	*.533-.611*	*.547-.634*	*.363-.621*
Buying Uni Degree	1	2	3	4	5
Low/No Corruption	.689	.606	.561	.565	.601
95% CI	*.653-.725*	*.572-.639*	*.531-.592*	*.533-.597*	*.500-.701*
High Corruption	.695	.619	.564	.547	.636
95% CI	*.650-.739*	*.584-656*	*.521-.606*	*.479-615*	*.539-.733*

Question Wording:

Corruption Condemnation is measured by the following question:

"Some people think that certain behaviors are always wrong, whereas others believe that there are occasions when breaking the rules may be justified. How wrong, if at all, do you consider the following behaviors to be?"

- "A public official asking for a favor or gift in return of services" (1=not wrong at all → 4=seriously wrong)
- "Buying a university degree that one has not earned" (1=not wrong at all → 4=seriously wrong)

The binary category in this probit estimation was composed of respondents who answered that these behaviors are "seriously wrong" (50.18 percent of the sample for the public official asking for a favor or gift; 60.32 percent for the university degree) versus those who chose the other response categories.

Note on the Analysis:

These predicted probabilities are calculated holding all predictors at their means, medians or modes (respondents who are female, of average age and education, in average household size and urbanity). Standard errors are clustered at the country level. Results are robust to pocketbook economic satisfaction.

Table B16. Huhe and Tang model in my sample: fixed effects OLS, 30 countries

	Model 1: Satisfied with financial situation	**Model 2: Satisfied with national economy**	**Model 3: Consumption Quartiles**	**Model 4: Ladder Question**	**Model 4: Life Satisfaction**
Economic Context (see column heading)					
2	11.739*** (1.539)	17.520*** (1.814)	0.439 (1.204)	8.385*** (1.409)	12.358*** (2.062)

3	18.112*** (1.556)	28.424*** (1.766)	-0.131 (1.213)	11.887*** (1.449)	19.033*** (2.012)
4	26.314*** (1.517)	37.708*** (1.729)	-4.066*** (1.229)	12.622*** (1.547)	29.086*** (1.943)
5	26.825*** (2.209)	37.859*** (2.225)	N/A	10.304*** (2.328)	30.868*** (2.271)
Anocracy	-6.600*** (1.940)	2.332 (2.124)	-10.220*** (1.551)	-8.713*** (1.964)	-1.402 (2.437)
Democracy	-35.399*** (1710)	-28.219*** (1.920)	-41.709*** (1.370)	-41.733*** (1.689)	-31.963*** (2.152)
Economic Context x Regime	Sat. with financial situation x Regime	Sat. with national economy x Regime	Consumption quartiles x Regime	Ladder question x Regime	Life Satisfaction x Regime
2 x Anoc	**-2.275 (1.872)**	**-9.406*** (2.038)**	**-3.159* (1.629)**	**-2.665 (1.823)**	**-4.274* (2.459)**
2 x Democ	**-3.759** (1.611)**	**-7.065*** (1.858)**	**-1.410 (1.300)**	**-3.921*** (1.497)**	**-4.805** (2.150)**
3 x Anoc	**-3.029 (1.902)**	**-10.826*** (2.049)**	**-0.472 (1.632)**	**-2.413 (1.876)**	**-4.435* (2.416)**
3 x Democ	**-4.759*** (1.629)**	**-8.817*** (1.826)**	**-1.033 (1.305)**	**-3.689** (1.545)**	**-4.787** (2.096)**
4 x Anoc	**-4.568** (1.894)**	**-13.070*** (2.085)**	**0.936 (1.642)**	**-1.480 (2.065)**	**-10.589*** (2.339)**
4 x Democ	**-7.570*** (1.604)**	**-11.840*** (1.822)**	**1.691 (1.315)**	**-0.059 (1.654)**	**-9.997*** (2.028)**
5 x Anoc	**-4.653 (3.063)**	**-10.865*** (3.153)**	**N/A**	**4.585 (3.325)**	**-9.621*** (2.956)**
5 x Democ	**-5.619** (2.404)**	**-12.619*** (2.471)**	**N/A**	**6.168** (2.523)**	**-8.339*** (2.420)**
Female	1.171*** (0.273)	0.639** (0.265)	1.099*** (0.307)	1.163*** (0.279)	0.997*** (0.273)
Age	-0.315*** (0.044)	-0.239*** (0.042)	-0.386*** (0.049)	-0.396*** (0.044)	-0.296*** (0.044)
Age^2	0.004*** (0.000)	0.003*** (0.000)	0.004*** (0.001)	0.005*** (0.000)	0.004*** (0.000)
Household Size	0.223** (0.093)	0.198** (0.091)	0.317*** (0.111)	0.111 (0.096)	0.071 (0.093)
Urban	-1.406*** (0.297)	-1.263*** (0.288)	-0.963*** (0.335)	-1.525*** (0.304)	-1.493*** (0.297)
Capital	-2.423*** (0.366)	-2.148*** (0.355)	-2.175*** (0.416)	-2.849*** (0.375)	-2.384*** (0.366)
Middle Ed	0.213 (0.359)	1.068*** (0.348)	1.329*** (0.405)	0.245 (0.369)	-0.064 (0.360)
Upper Ed	-0.256 (0.378)	1.193*** (0.364)	1.865*** (0.425)	0.035 (0.388)	-0.875** (0.380)
R Sq	0.321	0.362	0.269	0.284	0.317
N	28423	28294	24516	28671	28702

Note: Standard errors in parentheses. *p < 0.1, **p<0.05, ***p <0.01

Appendix C (Ch. 5)

Figure C1.

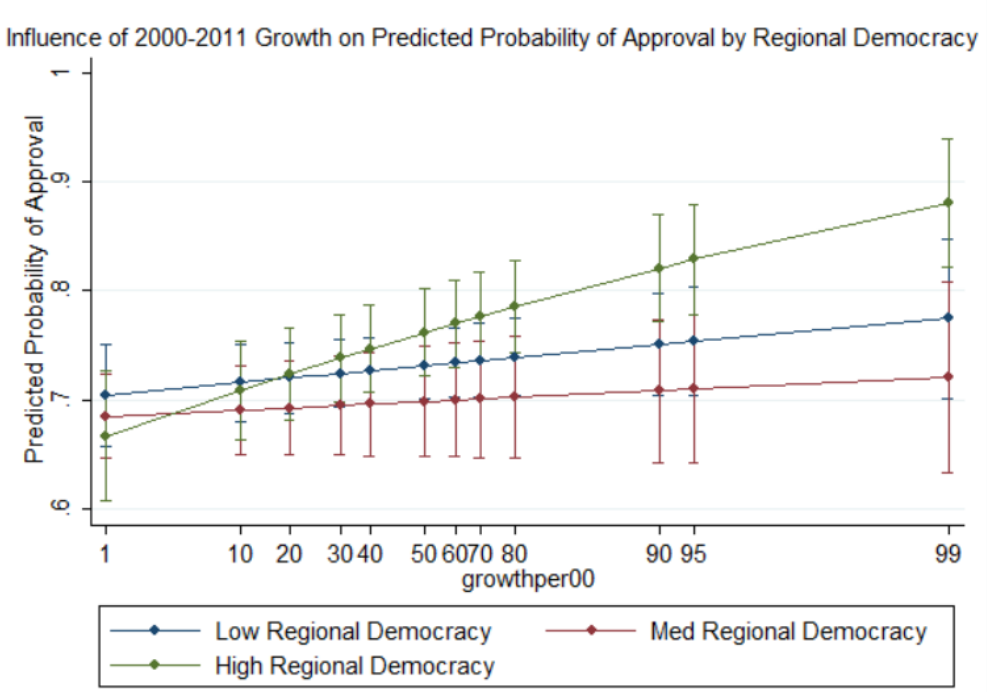


Figure C2.

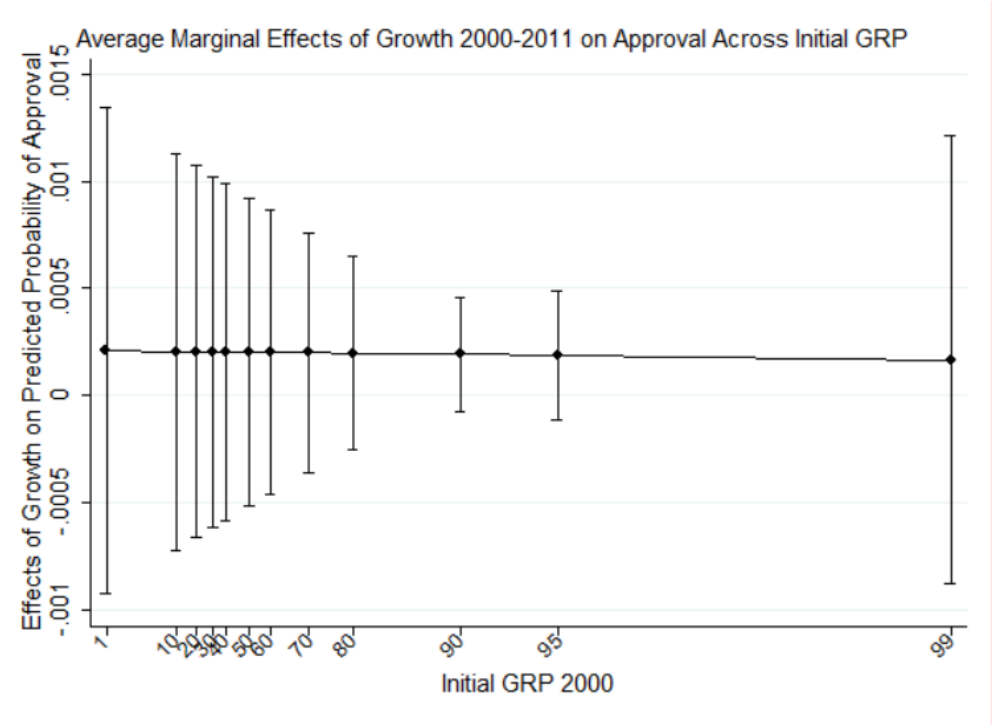


Figure C3.

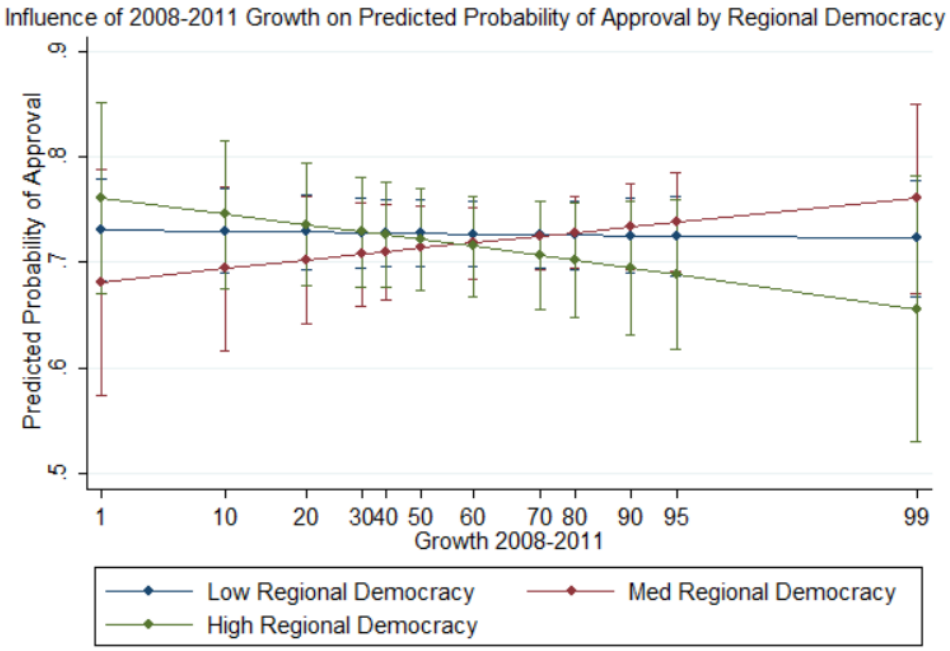


Figure C4.

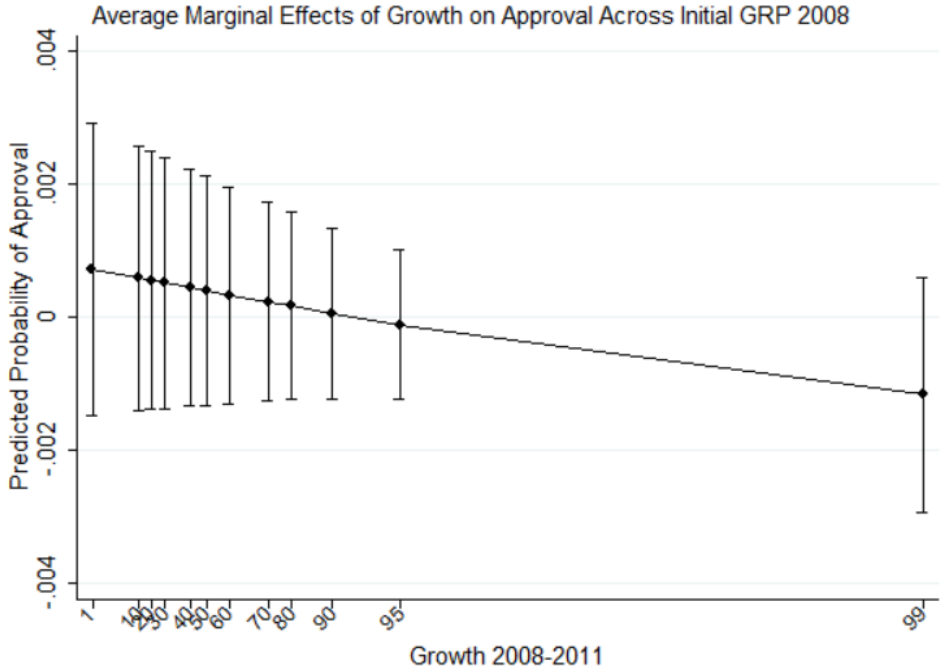
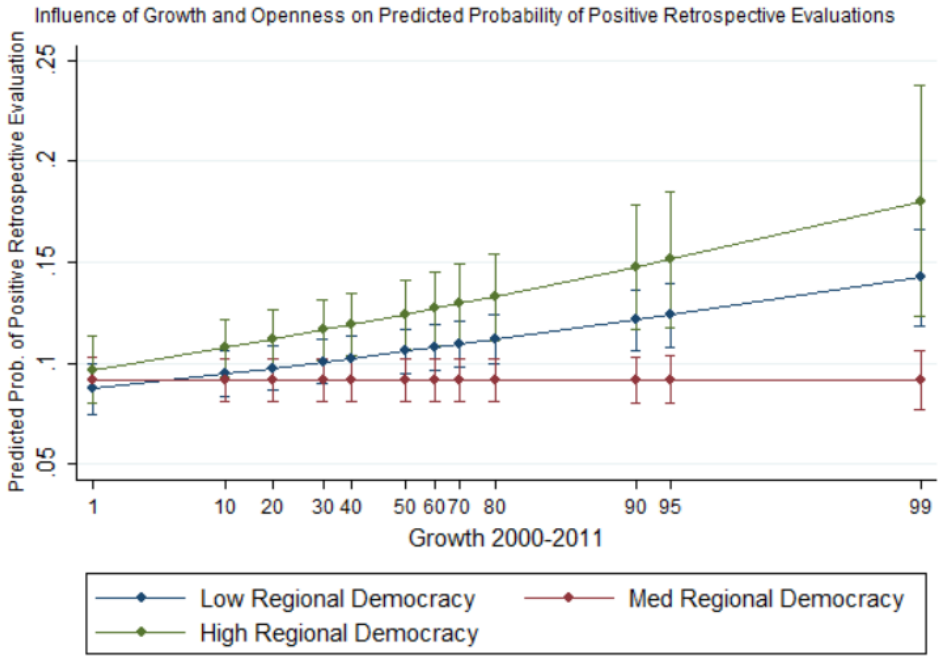


Figure C5.


Influence of Growth and Openness on Predicted Probability of Positive Retrospective Evaluations
Predicted Prob. of Positive Retrospective Evaluation
.05
.1
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10
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40
50
60
70
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95
99
Growth 2000-2011
Low Regional Democracy
Med Regional Democracy
High Regional Democracy

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